The Journal of HEREDITY

A RARE COLOR-MUTATION IN COLEUS

(SEE PAGE 293)

Is Sex-Control "A Scientific Reality"?

Children and Living Standards

Tumors in Twins

VOLUME 31 JUNE, 1940 NUMBER 6

The American Genetic Association

An incorporated organization devoted to promoting a knowledge of the laws of heredity and their application to the improvement of plants, animals, and human racial stocks

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The

Journal of Heredity

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CONTENTS

A Mendel Museum in America
By Gove Hambidge 259

Inheritance of Resistance of H-44 and Hope Wheats to Stem Rust
By R. F. Peterson 272

Jesse Baker Norton
By W. W. Gilbert 273

An Analysis of Tumors in Monozygous and Dizygous Twins
By Madge Thurlow Macklin 277

Inheritance of Pinkeye in the Fowl
By D. C. Warren 291

Genetic Studies of Coleus
By D. C. Rife 293

Special Eugenics Section

In cooperation with the American Eugenics Society

Responsibilities of Parenthood
By George W. Kosmak 297

The Significance of the Modern Eugenics Program
By Eugene L. Belisle 299

The Economics of Families with Varying Numbers of Children
By Frank Lorimer 300

Notes and Reviews

Education—The Real Eugenic Problem 263

Sex Control Again in the News (Rev. of Warren, *Animal Sex Control*)
By Robert C. Cook 265

"Race Hatreds"—Cultural or Genetic? 292

American Medicine is Told about Genetics 295

Number of Two-Child Families Increasing 304

STUDENT SUBSCRIPTIONS

The **Journal** has obvious advantages as a supplementary text in genetics classes. Since genetics deals mainly with form and pattern differences in organisms the **Journal** offers an unexcelled introduction to current developments, which have an added interest and appeal on account of the large number of illustrations published.

Some years ago we tried the experiment of offering the **Journal** at a special student group subscription rate for classroom use. The plan was tried the first year by several of our members, and some of them are still using it

STUDENT SUBSCRIPTIONS covering the academic year of nine months in groups of ten or more, may be ordered by members at one dollar ($1.00) each; four months for 50 cents. These special rates to students will be accepted only when applied for by members of the Association, in whose classes the **Journals** are to be used. The student subscriptions will be sent in bulk to the member in whose name the yare ordered. Orders should be sent to the Washington office of the Association.

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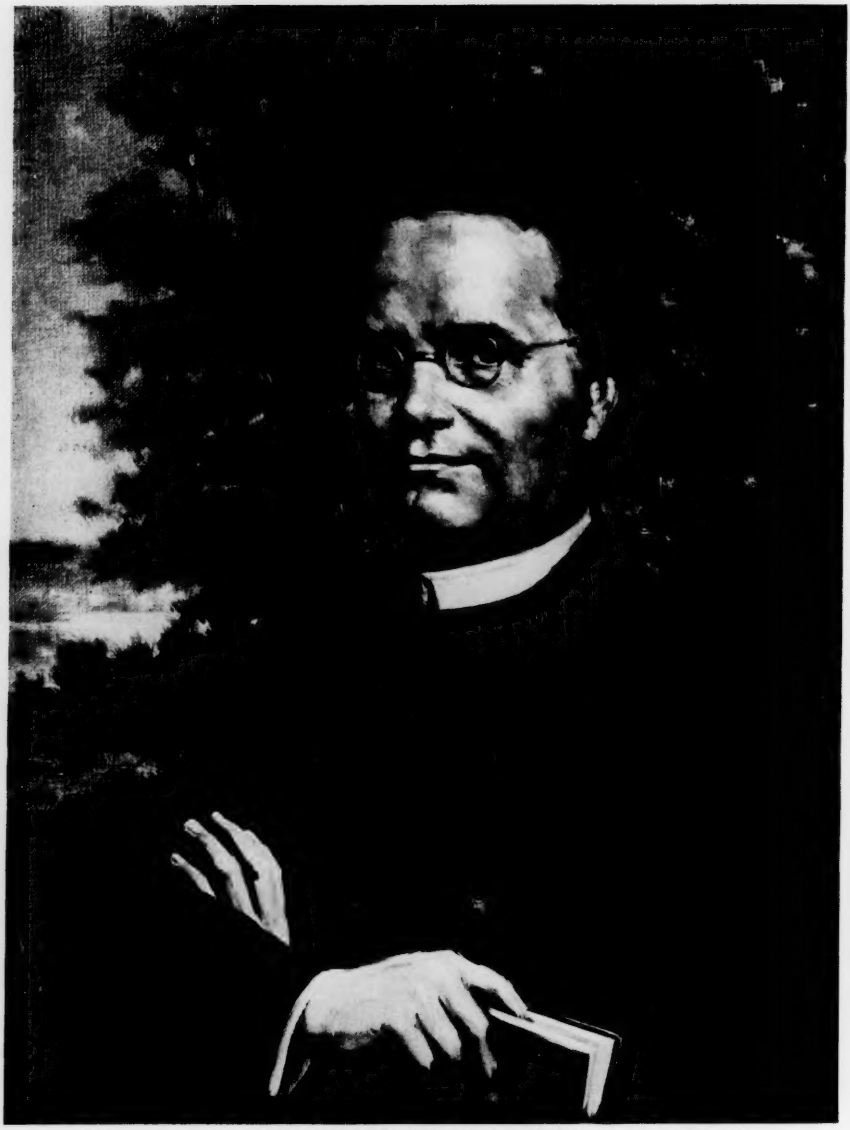


A NEW PORTRAIT OF MENDEL

Frontispiece

A portrait of Gregor Mendel painted by J. O. Flatter, of London. As background for the portrait Mr. Flatter has used a scene near Brno.

A MENDEL MUSEUM IN AMERICA

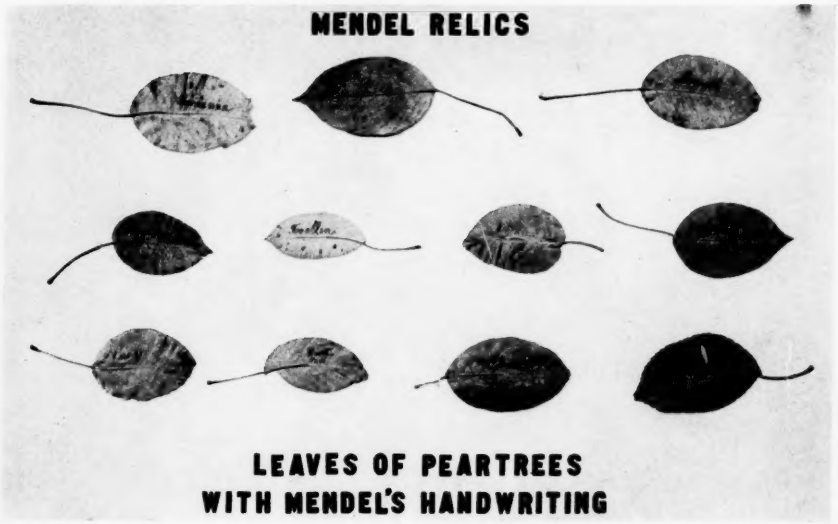


Figure 1

Pressed leaves of pear trees, bearing the varietal names in Mendel's handwriting. This is part of the collection of Mendeliana brought from Brno by Dr. Iltis to serve as a nucleus for an American Mendel Museum. Photographs by Hal Baird.

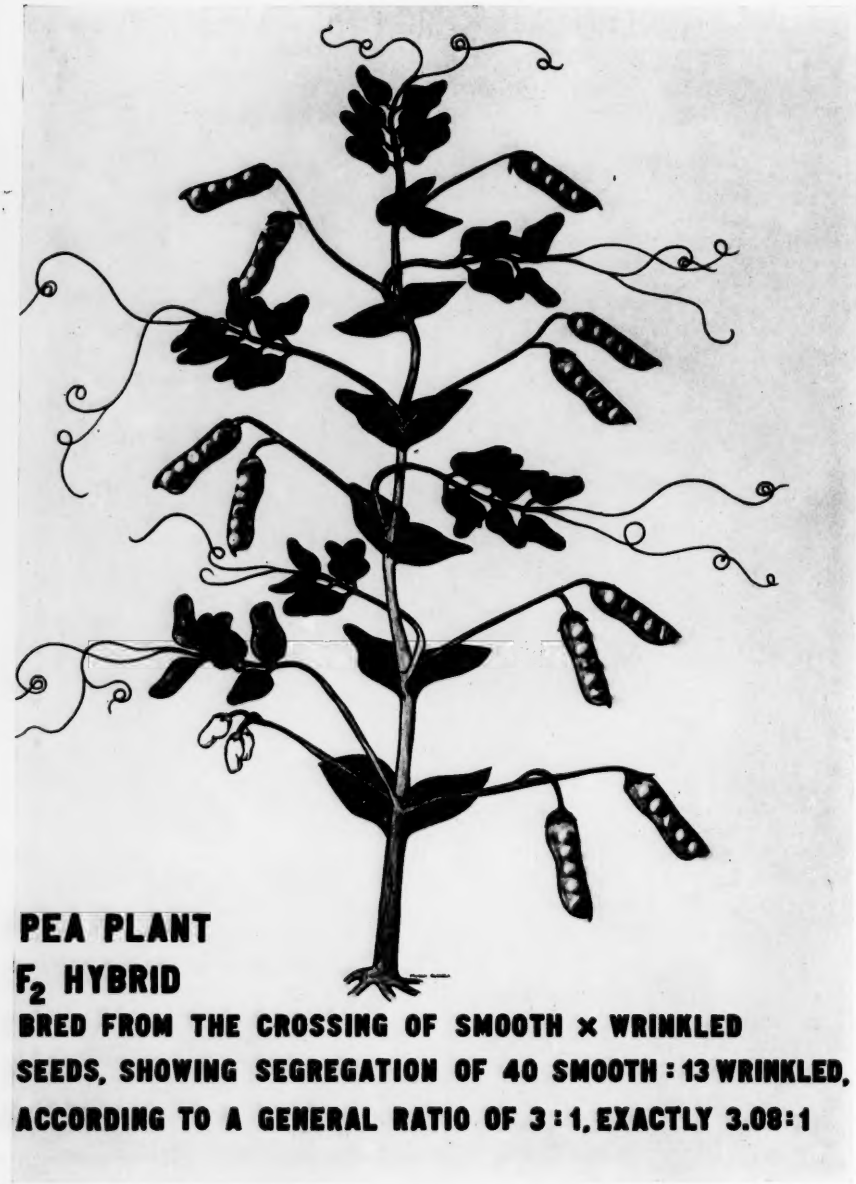
IN the flight of culture from dictatorship in Europe, the United States has been a haven for many brave spirits and fine minds.

One of the rebels against intolerable strictures on science, who fled when things became too hot for one with any love of freedom, is well known to geneticists in this country and many others. He is Dr. Hugo Iltis, the biographer of Mendel. Iltis was born in Mendel's home town, Brno, Czechoslovakia, and lived there for fifty years. Head of the "Volkshochschule," his passion was Mendeliana. Besides writing the only biography of Mendel, he founded and built up the only Mendel museum in the world. This little museum was a mecca for geneticists who happened to be traveling anywhere in those regions.

Iltis took issue with Hitler's racial theories and published what he thought. Then came the Brown Shirts. Just before they took over Czechoslovakia Iltis managed to get out with his family and what belongings he could hastily collect. He came to the United States, a middle-aged man, knowing practically no English, to start life all over again. One of the first things he did was to send in his application for naturalization papers.

Naturally Iltis had an immediate entrée among geneticists, who knew his work well. He learned English rapidly and quickly adapted himself to conditions in this country. After a period of hardship, he was generously offered a post as Professor of Biology at Mary Washington College, Fredericksburg, Virginia. He has been there a year and has won the admiration of the student body and the faculty for his rapid adjustment to new conditions.

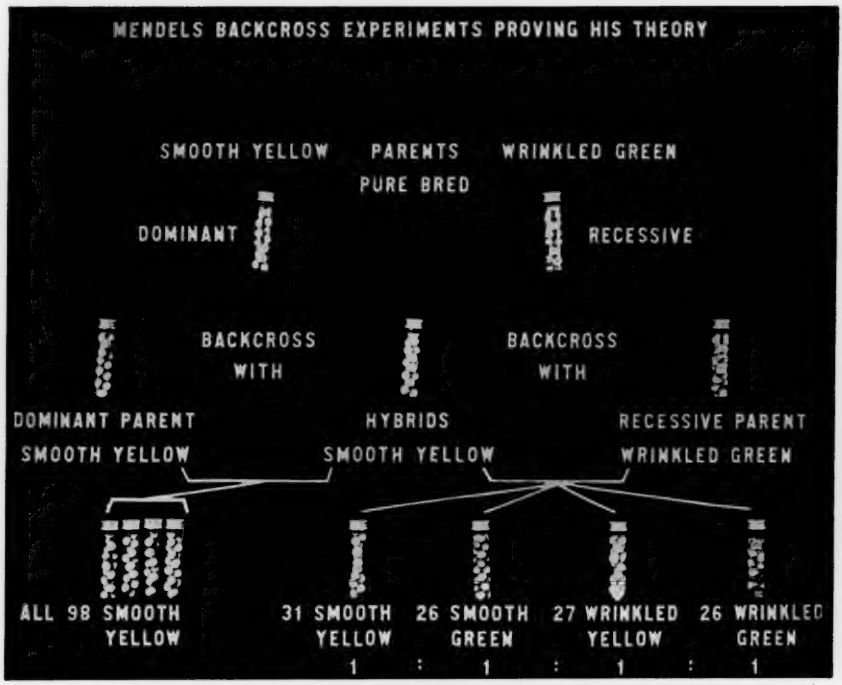
Dr. M. L. Combs, the President of Mary Washington College, did more than make a place for Iltis the teacher. He also made a place for Iltis the his-


PEA PLANT
F2 HYBRID
BRED FROM THE CROSSING OF SMOOTH × WRINKLED
SEEDS, SHOWING SEGREGATION OF 40 SMOOTH : 13 WRINKLED,
ACCORDING TO A GENERAL RATIO OF 3 : 1, EXACTLY 3.08 : 1

AN EXHIBIT TO SHOW ONE OF MENDEL'S CROSSES

Figure 2

Drawing of a pea vine with the pods represented as opened and with actual seeds from a second generation cross arranged in the pods. These show the segregation of wrinkled and smooth peas found in this cross. Prepared by Myran Russell, a student at Mary Washington College.



MENDEL PROVES HIS THEORY

Figure 3

A backcross test made by Mendel involving two pairs of Mendelian factors. The smooth yellow hybrid crossed to a parent having the two recessive allels, wrinkled green seed, produced a uniform smooth yellow-seeded progeny. When crossed to the double recessive parent of the original cross, statistically equal numbers of four phenotypes were obtained.

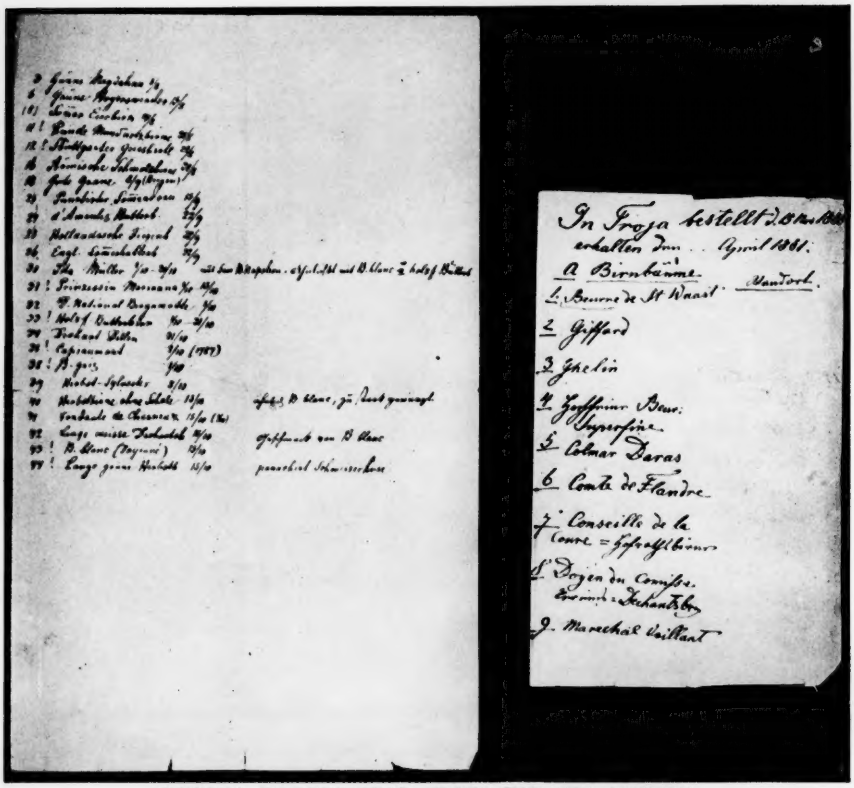
torian of genetics. Thereby Dr. Combs has put American geneticists in his debt. The purpose of this article is to tell this part of the story.

As most geneticists know, Iltis uncovered a unique collection of Mendeliana in the Augustinian Monastery at Brno where Mendel did his work. This included the original manuscripts of Mendel's papers on atomic heredity, in the *Proceedings of the Brunn Natural History Society,* Mendel's diary, letters from Mendel to his contemporaries and from them to him, and Mendel's notes on genetics, meteorology, horticulture, and bees. All of these became part of the Mendel museum, open to the public. Mendel's garden, of course, was also a show place in Brno.

When he left Brno, Iltis brought with him such of these Mendeliana as were his personal property. This nucleus is now the largest collection of Mendel relics outside of Brno.

Here is the point of the story. Iltis's life centered around his beloved museum. He has not for a moment lost this interest which gripped him throughout his life. When he came to the United States, it was with the dream of reestablishing, here in this free country, a true and permanent Mendel museum that would not run the risk of being wiped out by those interested only in inverting genetic theories so that they will give a pseudo-scientific background for ruthless oppression.

Through friends in this country, Iltis was able to enlist a good deal of interest



In Frojn bestellt d. Obst-Bäu
erhalten den April 1881.

A Birnbäume. Standort.

1. Beurre de St Waast
2. Giffard
3. Ghelin
4. Josephine Beur. Josephine
5. Colmar Daras
6. Comte de Flandre
7. Conseille de la Cour = Hofrath Beurr.
8. Doyen du Comice. Dechant Beurr.
9. Marechal Vaillant

MENDEL ORDERS PEAR TREES FOR HIS GARDEN

Figure 4

List in Mendel's own handwriting of varieties ordered by him in 1880 and in 1881. At this time Mendel was Abbot of the monastery at Brno and his days of genetic experimentation were over. He retained an interest in plants until the end of his life, in spite of the heavy demands on his time and energy made by administration of the institution he had been chosen to direct.

in his project. Finally a committee was organized that included Dr. C. B. Davenport, Dr. L. C. Dunn, Dr. E. W. Sinnott, Dr. C. E. Meyers, Dr. E. B. Babcock, Dr. Walter Landauer, Dr. W. V. Lambert, Robert Cook, and Gove Hambidge. The Honorable Henry A. Wallace agreed to serve as honorary chairman, though stating specifically that he practically always refuses such invitations because of his position as Secretary of Agriculture.

Meanwhile Dr. Combs gave the most practical assistance by making space available to Iltis for housing his collection at Mary Washington College, where in due time it could be opened to the public.

To make even the most modest beginning of a public exhibition, however, requires some funds for mounting, cabinets, and so on. Iltis's own slender finances, which he has devoted to this in so far as he could, were not enough. He has applied to two great foundations for a grant, so far without success. The College can do nothing without an appropriation by the Legislature, since it is a state institution.

The logical step seems to be a public

subscription to put the exhibit on its feet. Obviously the subscribing public will have to consist of geneticists and scientists especially interested in genetics. The lay public is not likely to rush in with subscriptions for a project such as this.

The project, it should be said, is on a much broader basis than the original museum in Brno. It should in fact ultimately turn out to be a genuine American Museum of Genetics, with material going far beyond Mendeliana and including up-to-date exhibits on all the important aspects of heredity. Iltis has been assiduously collecting such material from sources in the United States and adding it to his original Mendel collection. The center of that collection, by the way, is a portrait of Mendel recently painted by J. O. Flatter, an outstanding Austrian artist. In preparation for making this portrait, Flatter lived in Brno, studied all the pictures of Mendel, and met Mendel's relatives. Five out of the seven experiments on which Mendel based his work are also illustrated by material in Iltis's collection.

At a time such as this, when the whole world is torn by the violent emotions and the ghastly destruction of war, such a project as a genetics museum will seem unimportant and trivial to many people. Yet what is the reason for opposition to dictatorship and oppression in the modern world? Freedom, of course—and above all, to scientists, freedom of inquiry. If this freedom is crushed, hope for the future of civilization will be small indeed.

The genetics museum is a modest affair, though it might well be fairly important in the future, either in itself or as part of some greater museum. But modest as it is, it is one more beacon light of free scientific inquiry. These beacons are being extinguished in too many parts of the world. It is not a trivial matter to keep them glowing here in the United States: when the world holocaust is over, they will be badly needed.

GOVE HAMBIDGE

✦

EDUCATION—THE REAL EUGENIC PROBLEM

THE war in Europe has given further evidence about the proper aims of eugenics. It has not produced any new ideas, but has supported or confirmed the views previously held by the most thorough students of eugenics. The destructive forces have been going on for years, some of which are hereditary; they have been observed by careful students, but it has taken the war to bring them to a climax.

While eugenics must consider intelligence, the differences in average I. Q. between England, France, Sweden and Germany have been insignificant, but the I. Q. of Germany is significantly higher than that of any of the Balkans. It is not I. Q. that has caused the trouble. The differences between these nations are not easy to describe. There is no quality possessed by any one that is not possessed by all the others. But there are innate differences, a preponderance of qualities which can be seen by the careful observer, which has made it natural for each group to secure the type of leadership which it has.

It was the qualities of the German people that produced Hitler, but that is very different from saying that the German people wanted Hitler. No one who has known the German people in their own country or in ours can believe that they have secured the kind of government that they want. They might be compared to a person who built a fire in his house to keep warm, but failed to take proper precautions to prevent the fire from spreading. This is now the second time that the house is burning down.

Of the faults of the German people which have produced Hitler, first place should be given to the exaggerated place of the leader in the popular mind, and this fault is a growing one in America.

The average American is too much inclined to say that Hoover or Roosevelt is responsible for unemployment, or he will say that some man of his choice will lead us out. He is too reluctant to study the situation himself and ask the man at the head of the government, whoever he may be, to do as he is told. Carried to the extreme this is the attitude that produced dictatorship. It places full responsibility on the leader to produce the desired result, but cuts the cords by which he can be controlled. Then it is too late.

The writers of the American Constitution fully realized the dangers of giving the leaders too much power. Each group of the government was strictly limited as to what it could do without permission from some other body. For many years the restrictions were generally effective, but today the various branches of the government have exceeded their powers freely and with but slight checks from the courts. Public sentiment is clearly against strict control of its officials. It demands that its leaders produce the results regardless of law, a big step in the direction of getting a Fuehrer.

Why the change in feeling? We should make allowance for the change in conditions, and perhaps for a greater realization of the complexity of the problems, but quite evidently the changed attitude is deeper than this; it is due to a considerable extent to the changed composition of the people. The old Yankee population, with the others who refused to submit to the regulations in Europe, is now replaced, to a considerable extent, by others who found no fault with political or social restrictions, but came merely for better living conditions. The danger lies in this large group which will join any political or economic organization for the same reason that they came to this country, to get more material things for themselves. Of all degrees of I. Q., from the top to the bottom, they desire good government and properous conditions, but accept it without study of who or what has given them these advantages. They are the ones who seek the greater prosperity promised by a regimented society, which leads to dictatorship.

We now have a people of more nearly a central European composition, and headed for a central European condition of society and government. In this population, eugenics has a more important problem than that of conserving the I. Q. The professor who complains about the curtailment of academic freedom, while seeking a higher salary or greater economic security regardless of the academic freedom, is a more serious offender than the moron. He is building up a power by his acts contrary to his ideals.

In what direction can eugenics turn? First toward the reform of education, for there is no force in the community more obviously tending to perpetuate the subservient than the present plan of keeping youth in tutelage until they are mature, as a preparation for the higher occupations. Most destructive is the continued support of youth by their parents, or by charity and scholarships, through college or longer, when they should be forming habits of independent activity. There is not room here for even an outline of the changes in education that might be proposed. I will only point that from a eugenic point of view, all changes should be made in the direction of earlier independence.

This thought is nothing new. Students of eugenics have seen the need for the reorganization of education ever since there was any eugenics, but they have shied away, perhaps because it seemed too big a problem, because it touched too closely on their own occupations, or perhaps just because it was an unpopular thing to do. Now the war has shown the ultimate result of the present educational trend, and it will not be long before we see more clearly that we are headed the same way. It is time for advocates of eugenics to turn from minor issues like the sterilization of defectives to the more serious problems of preventing the sterilization of ideas and ideals on which a democratic eugenics might be based.

A. W. Forbes

Worcester, Mass.

SEX CONTROL AGAIN IN THE "*NEWS*"

Is the "Essential Scientific Question" Answered?

SUN spots, economic prosperity, and a multitude of other natural and human phenomena go in cycles. Sex cycles of many kinds have been charted and recorded by biologists. One sex cycle whose ebb and flow has never been adequately investigated is the perpetual cycle of interest in ways and means to pre-determine sex. Nearly two centuries ago Drelincourt (called Felincourt, by our latest author to put us right on sex) listed 262 theories of sex determination. Doubtless there were some duplications among these and many of the "new" theories of sex determination "discovered" since Drelincourt no doubt are reiterations of those on his two hundred year old list. In any event, the problem has challenged human ingenuity for a very long time.

Among recent theories, that advanced by Unterberger in 1930 has had a persistent popularity. In 1932, at the time of the Sixth International Genetics Congress, a "curbstone" account of Unterberger's theories, apparently delivered in New York City, captured perhaps more newspaper space than all of the actual Congress program put together. For months after that the "fan mail" of all agencies which might give information about sex determination bulged with inquiries and suggestions, offers of cooperation, and even with "proof" of the efficiency of one method or another. This flurry of interest in Unterberger's plan for controlling sex was discussed by Cole and Johansson in this JOURNAL in 1933.

On account of the nature of the material, any "method" of sex-determination, be it ever so silly and ever so far-fetched, will be "successful" half the time, because on the average boys and girls are born in approximately equal numbers. This fact, often overlooked by zealous sex-determiners, is undoubtedly an important factor in enabling so many kinds of sex-determining "experts" to get a hearing and even to allege considerable "proof". The failures, when they are admitted at all, can always be accounted for by some error in using the chosen technique.

The *News* Experiments

Within the last two years we have witnessed a new cycle of enthusiasm for the Unterberger acid-alkali technique. In 1938 the *New York News* was inspired with the idea that there was still informational gold in them thar sex-determinational hills, so an "expedition" was organized into this oft-fought over no-man's land. The *News* took a positive stand that Unterberger's experiments had constituted a real arrow pointing the way toward sex control, and that repetition of them with rats as the experimental material, would forever settle the question. This campaign of research and enlightenment (or vice versa) heralded the present sharp upswing in the sex-determination industry.

The progress of these experiments has been given exciting birth-by-birth publication in the *News*. The results have been summarized in a curiously sponsored book recently published.* The author is the "Sex Control Editor" of the *News*. The book is edited by the Director of the laboratories where the *News*' experiments were conducted. Thus the Editor supplied all the experimental data used by the author. The book chronicles a veritable laboratory blitzkrieg; where others had failed or had found indefinite or inconclusive answers, Durfee's experiments have been consistent, and the data presented are, on their face, conclusive.

It is claimed by Warren and Durfee that their experiments constitute a controlled repetition of Unterberger's work. This is not true, because Unterberger's initial claims were based on observations

*WARREN, CARL. *Animal Sex Control.* Edited by Thomas Durfee, Director of the Applied Research Laboratories. Pp. 174. $1.75, illustrated. Orange Judd Publishing Company, New York, 1940.

with human beings in the course of a medical practice. There was no laboratory control of the methods and of concentrations of acid and alkali used. Unterberger and Kirsch later attempted to adduce proof in a series of experiments with rabbits. Cole and Johansson pointed out in their 1933 critique the unsatisfactory and inconclusive nature of this work. Some of the later workers with the method used rats, but in the main their results were either definitely negative or not nearly as conclusive as the *News* experiments appeared to be. Both Unterberger and the other workers in these experiments used weaker solutions of acid and alkali than the Durfee group used. The *News* and Durfee have done a real service in setting up a standardized technique and a controlled experimental procedure in place of the rather ill defined and somewhat anecdotal experiments of Unterberger. Their results are now a matter of record so that they can be independently repeated. Until this has been done in other laboratories using the same methods and the same concentrations of acid and alkali, this method of determining sex in mammals has not been proven in an acceptable scientific sense.

Many geneticists and physiologists with whom the writer has discussed this question feel very strongly that it will not be possible to confirm Durfee's results for one or more of the reasons cited below. There is nothing internal in the material presented by Durfee and Warren which can make a statistical attack on it possible, and this widespread skepticism may be merely an emotional reaction against the altogether unscientific procedure followed in publicizing these experiments, for no reputable scientific laboratory will prejudice the final outcome of an experiment by an interim reporting of results, while an experiment is in progress. The reviewer is convinced that this skeptical attitude has a deeper basis than mere prejudice, and is grounded in a conviction that some other explanation for Durfee's results will eventually be found.

A Look at the "Record"

We are justified in appealing to a text thus authored and thus edited as demonstrating the background qualifications of those who have performed and sponsored this experiment. The misstatements noted below are definitely to be credited to a lack of acquaintance with the subject matter rather than to any deliberate purpose of misrepresentation.

On page 52 we read:

"All of the chromosomes in every egg are of the X variety, but there are two sorts of sperms. Fifty per cent of them contain 24 X-chromosomes. The other 50 per cent contain only 23 X-chromosomes plus the odd Y-chromosome. On a microscopic slide the sperms bearing Y-chromosomes actually can be distinguished and measured. They have heads smaller by 1/1,000 of a millimeter than those of the X-bearing group."

Obviously such an amazing assemblage of *X*-chromosomes ought to be powerful enough to put female secondary sexual characters on a billiard ball. The male sex can take considerable comfort in the ability of one *Y*-chromosome to dominate a whole nuclear harem of *X*'s and in spite of the odds against it, to set the developmental pattern definitely malewards. It still further adds to the confusion that the charts which accompany this amazing misstatement of the basic concept on which the entire book rests are somewhat inconsistently correct—showing but a single *X*-chromosome in each male cell—two in female cells. Even a competent proof reader should have smelled a rat in *that* wordpile.

The statement that sperm cells fall into two groups differing in size by a single micron and that this definite dichotomy is connected with sex determination is the ultimate balderdash. No doubt sperms could be divided into two groups, separated somewhere in the region of the midpoint of the entire size array. A bimodal distribution of sperm size has been reported in pathological material, but doubt is expressed whether it is normal. That this separation would divide the sperms into *X*-bearers, and

Y-bearers is pure inference utterly lacking any proof. The only way to test this would be to separate out a group of large or small sperms, using only one size for fertilization—a wholly impracticable procedure at the moment. The impossibility of direct proof is doubtless the reason Warren cites no authorities. Perhaps the author and the editor have gotten rats and men mixed up with Mangeldorf and Reeve's experiments with maize, where separation of starchy and waxy gametes was effected on a basis of size. Endosperm chemistry is not sex, and Mangeldorf's unique experiment rested on a solid observational basis. One did not need an interferometer to separate the pollen grains, a micro-sieve did the job. But this appears to be the only case on record where a separation of gametes on the basis of size has been successfully carried out.

Toward the end of the book (p. 162) when we get around to the philosophical implications this statement occurs: "It is not inconceivable, however, that the public welfare might well be served by some governmental or group influence upon the sex of citizens-to-be. One example will suffice. Certain diseases, such as haemophelia are transmitted solely by the females. Would it be unreasonable for the state to try to keep the progeny of stricken mothers exclusively male and thereby wipe out the disease in one generation?"

Aside from the fact that clinical cases of haemophilia are not known in women for obvious physiological reasons, the statement is wrong on several other counts. Haemophilia is transmitted by men as well as by women, if they live long enough to reproduce, for all male carriers of an affected *X*-chromosome show the disease. Were a haemophiliac woman to exist (as color-blind women occasionally do) she would have two affected *X*-chromosomes. Consequently all her sons would be affected. To wipe out the haemophilia in one generation by the procedure suggested by Warren it would be necessary to hit these sons mercifully over the head—or perhaps the idea is to let them bleed to death without issue, rather an elaborately sadistic procedure to kill *that* genetic cat. A workable method of sex control would make it possible to eliminate recessive sex-linked characters in one generation in the male line, for the sons of an affected male do not have or transmit the disease.

It may seem unfair to cite these outstanding solecisms as a preliminary to considering the claims made by Mr. Warren, Mr. Durfee, and the *New York News* regarding the Unterberger method. That the sex-control Editor should make such glaringly inaccurate statements regarding basic concepts of the genetic mechanism of sex-determination and that these should have been passed by Mr. Durfee as editor of the book can fairly be adduced as valid evidence of the state of biological and genetic sophistication of these experts. Their many forthright statements regarding genetics and sex physiology must be weighed in the light of these remarks.

The Experiments

So much for the background. In the book Warren gives us three stages in the evolution of the Unterberger theory: (1) Earliest developments in Germany, (2) confirmation in England, (3) and final proof in America.

The German angle has been gone into previously in the JOURNAL. Nothing very new has come out since 1933. In England the confirmation consists of the work of the two Taylors, gynecologists who have investigated in considerable detail the variations in the acid-alkali concentrations of the secretions of the female genital tract. They report the existence of a definite cycle in hydrogen-ion concentration, part of the cycle being acid in reaction, part alkaline. American workers have not been able to confirm the existence of a definite cycle though great variations in hydrogen-ion concentration are confirmed. These observations of the Taylors are put forward by Warren as being a secondary confirmation of the efficacy of the acid-alkali method, on the argument that all sex determination depends on this cycle —the time of conception in relation to

the acidity or alkalinity of the vaginal secretions determining sex. As a matter of fact, there are very strong reasons, not mentioned by Warren, for grave doubt that these observations and the assumptions based on them have any significance. This is discussed below.

The main part of the book is devoted to an account of the *News* sex-control experiments. The experimental approach consisted in commissioning The Applied Research Laboratories, located at Dayton, New Jersey, to undertake the laboratory tests which were forever to prove Unterberger right or wrong; preferably it was hoped, *right*. This Laboratory is "approved by the New Jersey State Board of Health as a Public Health Laboratory." Mr. Thomas Durfee, Director of this laboratory, has been in charge of the sex-determination experiments. First a series of preliminary experiments with rats were undertaken — using the acid-alkali as food and in douches. As many JOURNAL readers will recall, the Unterberger theory takes the view that a mildly alkaline medium is for some mysterious reason favorable to the survival and/or continued vigor and more rapid movement of sperm bearing a *Y*-chromosome, and a weak acid is equally selectively favorable to sperm bearing an *X*-chromosome. No direct evidence supporting this assumption exists. It bases on Unterberger's claim that a preponderance of boy babies followed sodium bicarbonate douches given to cure sterility. The assumption would seem to lend itself to a direct test: a sperm suspension in a mildly alkaline medium should show the relative immobilization of half the population, and a mildly acid medium should affect the other half of the sperm in the same way. No experiments cited in the book or known to the reviewer indicate that this has ever been seen to happen. Media deleterious or fatal to part of the sperms appear to affect them all about equally. Considerable work has been done on the effects of acid and alkaline media in connection with the study of contraceptives, but a differential effect such as that postulated seems not to have been recorded. The concentrations of acid or alkali used in these experiments have been reported to be immediately fatal on direct contact to mammalian sperms.

The Durfee experiments were widely publicized by the *News*, with the statement that were the desired results obtained, these experiments would constitute an independent confirmation of Unterberger's work. The series of articles in the *News* prior to the experiments and during the months they were in progress strongly hinted at the results which would be expected — that Unterberger's theory would be abundantly verified. Admittedly such a procedure should in no way influence the results of an experiment, although it does violate a basic fundemental of scientific method, which demands as much objectivity as possible in approaching any research problem: the truth is hard enough to trap under the most favorable circumstances. The research worker in physics, chemistry, or biology, who starts out with "desired results" and "undesired results" adds a weight of bias to his work which is a very real and very formidable subconscious danger. The "desired result" smacks of ancient magic and is of itself a perversion of the experimental method of arriving at the truth.

The records of Durfee's experiments have been given in detail in the *News'* articles and in Warren's book. Here also are given the details of the technique used. Essentially this consisted of a vaginal douche before mating of accurately titrated solutions of lactic acid or sodium bicarbonate (baking soda). Concentrations of both acid and alkali of one per cent and of two per cent and more were used. A concentration of five per cent prevented conception. The essential basis on which the Durfee experiments claim to have proven a practical method of sex control are two series of rat experiments and a series of rabbit experiments in which an "acid series" (female producing), an "alkaline series" (male producing) and a control series were run simultaneously. Two concentrations of solutions were used, one and two per

cent. The totals of the first experiment were:

EXPERIMENT 1 WITH RATS

Acid Series		*Alkali Series*	
♂	♀	♂	♀
16	65	48	16

On the second experiment, 100 females were separated into two groups. The totals for the two 50-litter series, using one per cent and two per cent solutions gave:

	Acid Series		*Alkali Series*	
	♂	♀	♂	♀
	44	127	135	31
	46	129	128	46
Totals	90:	256	263:	77
Sex ratios	35.2:	100	341.6:	100

CONTROLS

Acid Series	*Alkali Series*
96 ♂ : 100 ♀	100 ♂ : 100 ♀

Totals for the two rat experiments are:

	Acid Series		*Alkali Series*	
	♂	♀	♂	♀
	106	321	311	93

Rabbit experiments gave essentially the same results:

	Acid Series		*Alkali Series*	
	♂	♀	♂	♀
	28	93	100	24
Sex Ratios	30.1:	100	416:	100

If we accept the data at their face value there is no statistical basis for denying the validity of the reported results. The only feature of the data which gives any ground for suspicion is a curious fact that if we take the alkaline group (263 ♂ to 77 ♀ in experiment 2) and the acid group (90 ♂ : 256 ♀) the combined population is 353 to 333, or 105 males to 100 females, — exactly the normal sex-ratio of the rat. The total sex ratio for the two experiments combined is 101 ♂ : 100 ♀ practically that of the control group for this series. That the concentrations of acid and alkali used in these experiments should have had an exactly balanced efficacy, such that the combined litters have exactly cancelled out the plus and minus deviations is a curious coincidence. Such a result would inevitably be obtained if a series of normal litters were sorted out, placing the plus-male deviations in one series and the plus-female deviations in another. This is done below* for a series of 50 litters from an independent source.

This arrangement of the litters gives us two series which differ markedly in sex-ratio, and with the total sex-ratio is skewed slightly femaleward from the normal, namely 100 ♂ : 100 ♀ instead of 105 ♂ : 100 ♀. These ratios are not as extreme as those reported in the Durfee experiments. While in this series there is only a single one-sex litter, nine all-male litters were reported in Durfee's second alkaline experiment, and four all-female in the acid series. Thus there are differences of distribution of sexes within the litters in Durfee's series and in normal litters.

The fact that the *News'* series totals up to a normal sex-ratio does not prove anything. Added to the more weighty objections below it is perhaps an added

***TABLE I. Sex Ratio of Normal Litters.**

From Jordan, H. E., and W. H. Paine. *Amer. Nat.* 64:425. 1930.

PLUS FEMALE			PLUS MALE		
Litter No.	Male	Female	Litter No.	Male	Female
C-51	3	8	A-61	9	3
A-57	5	7	A-11	8	6
B-4	2	2	A-12	8	6
A-13	5	7	A-14	8	6
A-15	4	5	B-18	3	2
A-16	6	8	B-19	9	4
B-21	1	5	B-20	5	4
A-22	5	6	A-24	10	2
A-23	4	5	A-28	6	6
A-32	2	2	C-34	6	5
A-33	1	2	B-35	4	3
C-39	1	6	A-36	5	5
A-44	4	6	A-37	5	4
A-46	5	5	B-38	7	6
A-48	2	8	A-41	8	7
A-56	6	7	A-45	7	4
A-54	4	5	A-47	5	5
A-55	2	5	B-49	5	4
A-63	5	5	A-67	1	0
B-66	4	10	B-53	7	4
B-79	6	7	A-58	4	3
A-82	4	6	A-59	7	5
A-84	3	6	A-60	6	1
A-69	6	7	A-64	6	5
A-73	1	9	B-78	6	6
	102	149		155	106

Sex ratio 69 ♂ : 100 ♀ Sex ratio 148 ♂ : 100 ♀

	102	149
	155	106
	257 ♂	: 255 ♀

Total ratio 100+ ♂ : 100 ♀

reason for withholding final acceptance until independent confirmation of these experiments has been forthcoming.

The Physiological Objections

Abundantly established facts of sex physiology make it difficult to understand how the method would operate. These objections are so well grounded as to justify looking upon this technique with suspicion until further confirmation through repetition of the experiments has been forthcoming.

The claim that a variation in the hydrogen-ion concentration of the vaginal secretions controls the sex-ratio in nature suffers from very extensive conflicting evidence. The work of Hartman and other physiologists has shown that ovulation occurs at a very definite point in the sex-cycle. Furthermore, many workers have established the fact that spermatozoa survive only for a short time in the female reproductive tract.* Guttmacher and Shettles (*Human Fertility*, March 1940) have recently shown in women that both the vaginal and cervical secretions are actually spermicidal except during a very brief period in the monthly cycle. Thus a large and increasing weight of evidence is accumulating that conception can normally occur during only a small sector of the monthly cycle. If this is true it is impossible to understand how the hydrogen-ion concentration of the vagina during most of the cycle can have any effect on the sex ratio. Warren's claim that the validity of Unterberger's concept is proved through the work of the Taylors seems to border on the phantastic when the above facts are all taken into account.

The procedure itself suffers from some curious paradoxes. While the acid douches would unquestionably be fatal to spermatozoa and the alkaline douches very probably so, it remains very much of a question whether they would have much influence on the state of vaginal acidity or alkalinity for an appreciable time after the douching was performed. Those who have worked with rats emphasize that there is almost invariably a considerable interval after such treatment before mating will take place. Durfee states that "if mating had not occurred in two hours the douches were repeated," so that it is clear that a very considerable interval usually intervened between treatment and mating. The mammalian body is well enough buffered, so that only a transitory effect is to be expected. Durfee makes no mention of any attempt to check this point, and in the rat it appears below that the state of vaginal acidity probably is of very secondary importance in any event.

That in the rat enormous numbers of sperm are immediately ingested by the uterus (some workers even holding that

*As a running commentary to this whole problem the reviewer quotes from Hartman's recent exhaustive review of this field [*Sex and Internal Secretions* (edited by Edgar Allen) pp. 630-719. Wiiliams and Wilkins, second edition, 1939]: Here the established facts regarding sex physiology in mammals are reviewed in detail, with copious references to the literature. A few of the statements there made on the basis of much more careful research than the *News* experiments, and which cumulatively constitute a very serious—if not fatal—barrier to accepting Durfee's claims are quoted herewith: (p. 681) "Human sperms endure 0.5 per cent lactic acid, a normal constituent of semen which increases on standing; but 1 per cent lactic acid kills sperm. No such concentration is ever attained in the human vagina of healthy women; hence the prescription of alkaline douches in sterility cases is theoretically unsound. The normal vagina is, indeed, most acid in the middle of the menstrual cycle when conception usually takes place." * * * * (p. 687) "Sperms are never found in large numbers in the fallopian tube. Nevertheless Hammond (1934) has shown that the ova are not fertilized by the vanguard of sperms first reaching the ova but by members of the 'swarm' that have assembled several hours later." * * * * (p. 698) "In some species the passage of semen into the uterus during copulation is almost instantaneous. Thus, in the rat Hartman and Ball (1930) made the rather surprising observation that 30 seconds suffice for the transfer *en masse* of enormous numbers of sperms into the uterus. That entire blocks of semen are thus ingested by the uterus and that the mixture of the semen with the uterine fluid is effected by the contractions of the uterine wall was clearly demonstrated by Rossman (1937) who made direct observations of the intact organ by the transillumination method of Kinsely and checked these with an histological study of frozen preparations." * * * *

they are ejaculated directly into that organ), where the environment is normally alkaline is a fact confirmed by several workers. Since the vaginal douches used by Durfee could hardly have affected the reaction of the already alkaline uterus into which the spermatozoa are introduced directly during copulation, the whole theoretical basis for the method seems very precarious.

If Durfee's experiments should be independently confirmed it will be necessary to re-examine critically a great mass of very careful experiments regarding sex physiology in mammals, especially in the lowly rat, whose sex life has been more completely studied than even that of the primates. But so well grounded are the observations cited above, that Durfee is over-optimistic when he says that the "essential scientific question is answered. We have controlled the determination of sex in rats and rabbits. The tasks ahead are those of perfection and adaptation only." One task which Durfee and Warren have not even begun is to fit their experiments into the far more extensive, consistent, and critically conducted experiments cited by Hartman. Until that is done, Durfee has presented a problem: he has not solved one. Either something is wrong with the work of many outstanding physiologists or an unsuspected principle is at work. Science does not tolerate a paradox. Only when the whole story is told do we get a true picture. And until this is done "the essential scientific question" is definitely not answered.

In expressing such doubts of the validity of the experiments performed by Mr. Durfee and reported by Mr. Warren, it must be made plain that we are in possession of no information casting any reflection on their integrity. In more than one instance in experimental biology equally sincere workers have had to admit unfortunate errors beyond their control. There is also always the possibility of weird coincidence. Somebody always wins the sweepstakes Grand Prize, even though the calculus of probability gives the winner considerably less than a ghost of a chance.

The odds that chance deviations account for the series reported in the *News'* experiments are also very great but such a possibility exists. It is urgently to be hoped that a repetition of Durfee's experiments will be undertaken in one or more independent laboratories using exactly the same techniques. If the tests confirm Durfee's findings, then no competent biologist will deny that the *New York News* has indeed pioneered a method which makes possible the determination of sex in mammals. Until this confirmation of the Durfee experiments, using similar materials, similar techniques and similar concentration of acid and alkali, the Unterberger method still suffers from the difficulty that repetition of any single experiment undertaken as the result of Unterberger's report has not been made, or has been inconclusive. As Cole and Johansson pointed out in 1933, attempts to confirm the method with small animals have not been successful nor were the experiments run at that time by Cole and Johansson confirmatory of the method. The objections put forward above are admittedly not positive proof that Unterberger's method could not work, though cumulatively they give ample ground for continued doubt. The objective approach of science requires that judgment be suspended until independent confirmation or denial has been published.

The genetic world will eagerly await news of such independent tests. We understand that at least three laboratories are undertaking to check Durfee's results. Publication of their conclusions will differ from the *News'* technique in that no statement can be expected until these experiments are complete. When that time arrives will be soon enough for the genetic world either to accept unqualifiedly or to reject finally the claim that by these means sex determination in mammals is at present a realizable possibility. The reviewer will be the first to admit that he is wrong, when such confirmation is published, and he would deem it a privilege to publish it in this JOURNAL. But it will astonish him considerably if he is called on to admit the one and do the other.—ROBERT COOK.

INHERITANCE OF RESISTANCE OF H-44 AND HOPE WHEATS TO STEM RUST*

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DR. E. B. BABCOCK[1] quoting a letter of E. S. McFadden has given plant-breeders valuable information on the chronology of Hope and H-44 wheats which were developed by McFadden[1, 6] from a cross made in 1916 between Marquis wheat and Yaroslav emmer, and distributed for use as breeding material in 1925. This clarification of the history of these two varieties is timely, since most of the rust resistant spring wheat varieties now being shown commercially or increased for distribution in North America are derivatives of either Hope or H-44. The author would like to add to the points already covered an observation on the early work on the inheritance of field resistance to stem rust of the varieties under consideration.

Various reviews of this work give the impression that the inheritance of the resistance of H-44 or of Hope to stem rust under field conditions was first placed on a definite factorial basis in 1928. However, in 1927 Goulden, Neatby and Welsh[4] reported that in a study of 1,000 F_3 families of the cross H-44 × Marquis, field resistance was found to be governed by one pair of factors, and that it was inherited independently of seedling resistance. A full report of this work by the same authors[5] appeared in 1928.

In a paper of Clark and Ausemus[2] in 1928 the field rust reactions of F_2 populations of three Hope crosses grown in 1927 failed to show any definite genetic ratios but the near-immunity of Hope was shown to be dominant. Later in 1928, at the annual meeting of the American Society of Agronomy, the same authors presented F_2 and F_3 data indicating a two-factor basis for the inheritance of near-immunity in crosses of Hope with Marquis and Reliance, and a single-factor basis in the Hope-Ceres cross. These results were summarized by Clark and Humphrey.[3]

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JESSE BAKER NORTON

1877-1938

JESSE BAKER NORTON died June 8, 1938, in a hospital at San Diego, Calif. after a long illness. This brief contribution is being offered now since no full statement of his work and accomplishments has appeared; it has been largely contributed by his friends in response to my request.

Mr. Norton was born at Leadvale, Tenn. on August 19, 1877, and graduated from the Kansas Agricultural College in 1897, receiving the degree of M. S. in 1901. In 1900 he was appointed a scientific aide in the United States Department of Agriculture and later in the year went to the Kansas Agricultural Experiment Station as assistant entomologist, serving during the years 1900 to 1902. He was an assistant physiologist in the Bureau of Plant Industry, U. S. Department of Agriculture, from 1902 to 1907; asst. professor of plant breeding at Cornell University, 1907-1908; physiologist in the Bureau of Plant Industry, 1908-1918, and agricultural explorer during 1919-1920. He joined the Coker Pedigreed Seed Company in 1920 and continued with it until 1926, when he became physiologist of the Associated Chambers Plant Pest Control Committee of Imperial Valley.

Norton was a member of the American Association for the Advancement of Science, American Genetic Association, Dairy Science Association, American Geographic Society, Washington Academy of Science, and the Washington Biological Society.

During his early connection with the United States Department of Agriculture as plant physiologist, he came under the influence of Dr. H. J. Webber, then in charge of the Plant Breeding work for the Department, and formed an association that led to his following Dr. Webber to Cornell in 1907, as an assistant professor in the latter's department there.

Dr. Webber, now emeritus professor of subtropical horticulture, at the University of California, has very kindly supplied the following regarding his early association with Mr. Norton:

> I valued my personal association with J. B. mainly because of the influence of his keen analytical mind and his frank though kindly criticism. He never hesitated to tell me just what he thought of any of my schemes, ideas, or manuscripts in plain unvarnished words, and he was usually right. This was the character that influenced me to take him to Cornell University with me as an Assistant Professor in the Department of Plant Biology when it was first organized. I felt that he possessed just the traits I needed in an associate. . . .
>
> He only remained a little over a year in Cornell, 1906-1908; but he had a marked influence in shaping the plans of the Department and outlining the methods of cereal breeding that were followed successfully for many years. He missed the life in Washington, D. C., and returned to the United States Department of Agriculture. His leaving was not because of any trouble or disappointment, but simply because he liked the life in Washington better than that at Cornell.
>
> On his return to Washington he again took up the asparagus breeding work that finally, after long years of work, resulted mainly in the one great contribution or bulletin which was a masterpiece of thoroughness and achievement. No greater achievement in any other crop has ever been attained by an individual. His series of Washington asparagus varieties (George and Martha) are still maintained as pure lines by the Pedigreed Seed Company, Hartsville, S. C. and are still, I believe, the most excellent varieties available. I look upon that bulletin of J. B's. as outlining the most perfect system of plant breeding that has ever been devised; and, of course, it is only possible with such a dioecious plant which is perennial and produces an abundance of seed, so that controlled parentage on a large scale becomes possible. It would be entirely possible to have all asparagus grown in the world produced by the mating of the two best plants. The possibilities are further enhanced by the fact that each plant can be indefinitely propagated by separating the root sprouts.
>
> As a young man in the United States Department of Agriculture, and at Cornell, J. B. was a hard worker, keenly scientific and stimulating as an associate. I next came in contact with him in 1920-1921 when for a short period I became General Manager of The Pedigreed Seed Company at Hartsville, S. C. Here, Norton's services were very valuable as his critical attitude kept the Company stirred up

to a higher degree of accuracy than would otherwise have been possible.

Here his work, while mainly on asparagus, had to do also with grains and to some extent with cotton and other crops, photography and pest control. During this period, 1920-1922, my son, Dr. J. M. Webber, was getting to university age and his association with Norton proved exceptionally stimulating. They made numerous collecting trips together and my son still gives Norton credit for having more influence in getting him started right than any other associate.

The high regard in which Norton was held in his later years by his employers, David Coker of The Pedigreed Seed Company, and Mr. B. A. Harrigan, Imperial County Agricultural Commissioner, testified to the character of his ability and integrity. His greatest contribution to life in my judgement was the stimulating influence he exercised on his associates.

JESSE BAKER NORTON

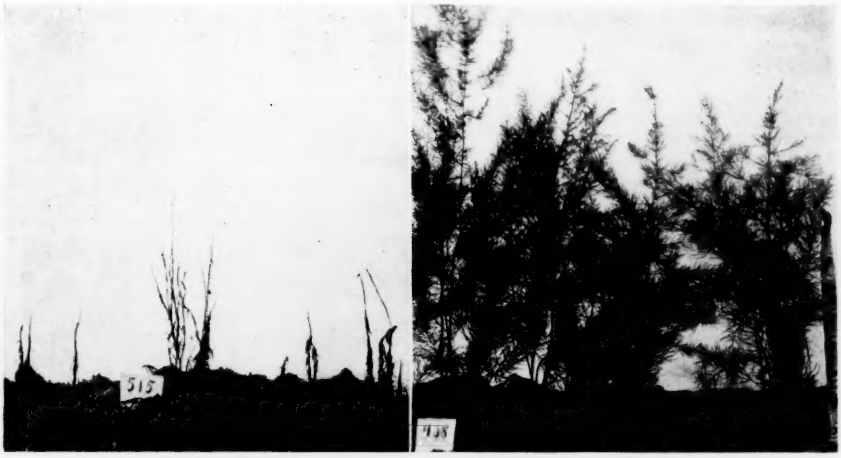
Figure 5

Portrait made about the time that Norton was engaged in his asparagus breeding work in the U. S. Department of Agriculture.

In the period from 1908 to 1918, during which Norton was almost continuously connected with the United States Department of Agriculture, he was mainly occupied with asparagus breeding. In this work he collected many foreign strains of asparagus, which were tested at Concord, Mass., in cooperation with the Massachusetts Agricultural Experiment Station. From this material he undertook to breed rust resistant strains of asparagus. He was successful in developing a method of breeding for rust resistance and by this means he originated the Washington strains of asparagus. These were rapidly distributed to all asparagus growing regions of this country and today they still constitute the most widely grown and most valuable commercial varieties. His work in this field and his methods of breeding and selection were published in 1913, in *Bulletin 263* of the Bureau of Plant Industry.

This highly successful project from the practical point of view is noteworthy in that it represented a very early application of a method of plant breeding which has been increasingly used in recent years, and which promises to be one of the important methods of the future, —the combining of genetics and plant introduction. Before Norton's work, inheritance of disease resistance in breeding had been used to a limited extent, and plant introduction had discovered and made available new crops and desirable varieties which had proved to be important contributions to American agriculture. Norton combined these two techniques in using the material accumulated by plant introduction to make available for his breeding work the widest possible range of the characters in which he was interested.

This conscious shift in plant introduction objectives from the discovery of adapted varieties as such to a search for genetic characters, marked a very important advance. In recent years it is being recognized that here lies one of the most significant services of the plant explorer—the discovery of new genes rather than of new varieties, so that the plant breeder can have at his disposal



SUSCEPTIBLE AND RUST-RESISTANT ASPARAGUS

Figure 6

Norton's great contribution to plant breeding and to American agriculture was the production of rust resistant strains of asparagus, which he produced at a time when this disease threatened to wipe out American asparagus growers. Norton's approach, to assemble as great a collection of different strains of asparagus as possible to serve as the basis for his breeding work, marks a use of plant introduction material which has had outstanding success in later years and in the hands of other workers.

the maximum range of characters within a species, or even in an entire genus.

From Mr. B. Y. Morrison, Principal Horticulturist in Charge, Division of Plant Exploration and Introduction, comes a brief report of the trip Norton made for that Division to China in 1919. Leaving Washington in April, he sailed from San Francisco to Vladivostock on May 5, made a short stop at Nagasaki, Japan, and thence by way of Shanghai, through Foochow to Kuliang. He returned in September, leaving Shanghai for Seattle in October. Norton as a result of this trip prepared "A Preliminary List of Plants Known to Occur in the Province of Fukien."

The introductions made by Norton appear to be limited to those recorded in United States Department of Agriculture "Inventory of Seeds and Plants Imported," No. 61, under Plant Introduction numbers 48,551 to 48,586 and 48,689 to 48,750. These include a great range of material, the major part vegetables or fruit, particularly pears and plums. Among the material were some forage crops and vegetables that proved to be of special value in breeding work carried on by Norton after he left the Department and joined the Pedigreed Seed Company.

Among the introductions made at this time was a Korean lespedeza believed to be a separate strain of *Lespedeza striata*. This was purchased by Mr. Norton at Kobe on his return journey. It has proved of exceptional value in soil conservation work.

After he returned from China in 1920, Norton left the Department to accept employment as a plant breeder with The Pedigreed Seed Company of Hartsville, S. C. This Company, with Mr. D. R. Coker as president, had for some time been particularly interested in the further development and distribution of the Martha and Mary Washington rust-resistant strains of asparagus originated by Norton.

In reply to a letter requesting him to contribute something relative to Nor-

ton's work during the years spent in his employ Mr. Coker wrote in part as follows:

Never have I known a man of such wide scientific and cultural interests, of such broad knowledge. He could illumine practically any subject upon which he was asked to express himself.

Shortly after writing this and promising further information concerning Norton's work with the Pedigreed Seed Company, Mr. Coker died. Mr. Coker's successor as President of the Company, Mr. George J. Wilds, gave the following details of Norton's work:

The late J. B. Norton's contributions to our program were such as to belie my feeble efforts. Dr. Norton was an institution in himself to us. No man that we have ever been associated with had such a profound store of knowledge. We consulted him and accepted his advice on many of our breeding problems. Some of his direct contributions are roughly catalogued below:

1. He made the original crosses from which our Norton, cold resistant oats, were bred;

2. From a cross of Redhart Strain 1 and Golden Chaff made by Norton our Redhart wheat, Strains 2, 3, 4 and 5 were bred;

3. He devoted much time to the selection of our Darlington County watermelon which is accepted as the best quality melon grown in South Carolina;

4. He carried on extensive breeding experiments with cantaloups. We are growing today a superior quality, thick meated, spicy melon that we call the Norton melon;

5. He bred numerous types of early maturing cajens (pigeon peas) from the perennial type;

6. He kept accurate weather records, during the entire period that he was with us. From these data numerous charts were made and correlations worked out;

7. He was intensely interested in our purebred Guernsey herd. He delved deeply or not at all. While working out the pedigrees of this herd he became interested in the entire Guernsey breed and at one time, as stated by Guernsey officials, he was the best informed man in the United States on the Guernsey breed; and

8. When the boll weevil arrived, he conducted numerous cage experiments and largely through his efforts the 1-1-1 calcium arsenate, molasses and water, early poison mixture was worked out and adopted.

I once heard Dr. Webber state that Dr. Norton was the best critic that he knew. We found him to be this and a very dearly beloved friend. His services to our institution cannot be measured by these brief remarks. A volume might be written and yet his services not covered.

The last twelve years of Norton's life were spent in a field of activity outside the scope of this JOURNAL. An account of it would only draw emphasis from his major contribution: pioneering a technique for breeding immunity or resistance to many plant pathogens and which has revolutionized the work of the plant explorer. The profound importance of this technique can be realized when we consider the changed emphasis in plant introduction in 1908 and today. Then a plant explorer's success was measured largely by the wealth and variety of material which reached these shores and survived. Today a plant exploration expedition can be counted a success if a single plant resistant to a particular disease, but otherwise worthless—is brought back. And expeditions in search of a single plant are justified abundantly by the results. The value of this technique is attested in saving more than one agricultural industry. It stems directly from Norton's asparagus breeding methods—and this contribution assures him a permanent place in the living history of the plant breeder's art.

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AN ANALYSIS OF TUMORS IN MONOZYGOUS AND DIZYGOUS TWINS

With a Report of Fifteen Unpublished Cases*

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TWINS are of at least two varieties: (1) those from a single fertilized zygote, and so with identical hereditary potentialities, and (2) those from two fertilized zygotes, and hence with hereditary potentialities differing as greatly as those of sibs. The comparison of similarities in identical twins with similarities in fraternal or dizygous twins thus affords a clue to the rôle played by heredity in producing certain traits or characters. If monozygous twins are much more alike than are dizygous twins with respect to the appearance of a character such as blood grouping, then we may conclude that blood grouping is dependent upon hereditary factors. If monozygous twins are not any more alike than are dizygous twins in the history of measles attacks, then we may conclude that the infectious agent of measles, rather than an inherited susceptibility, is the predominant cause of a measles attack.

Similarity in Type of Tumor in One-Egg and Two-Egg Twins

If tumors are dependent wholly or in large part upon hereditary factors, then we should find identical twins resembling each other with respect to the presence or absence of tumor more often than they differ in this regard; moreover, they should exhibit the same type of tumor far more frequently than they exhibit diverse types of tumors. They should be much more alike in these two respects than are dizygous twins. Conversely, if we find monozygous twins being similarly affected with tumor in greater proportion than are dizygous twins, we may conclude that the presence of tumors, and the type of tumor are dependent in large part at least, on inherited factors.

The criticism has been made, however, of studies of pathological conditions in twins, that the records taken from the literature are not representative of the whole twin population. The first objection is that cases are more liable to be recorded if both twins show the condition than if one twin only is affected. This criticism is probably quite justified, but it is robbed of some of its potency, when we remember that the same degree of selection of cases will be taking place in fraternal or dizygous twins, so that the dizygous twin pairs in which both are affected will be also unduly frequently reported. If the condition is one wholly or partly conditioned by inherited factors, a series collected because *both* twins have been affected should show an undue number of monozygous twin pairs in the series. This is exactly what happens in the cases reported in the literature, and there are about 2½ times as many instances of *both* monovular twins affected with tumor as there are of *both* binovular twins.

The second objection is that because such pairs as have both affected are more often reported, concordance in tumors in monovular twins is shown in far higher percentage than it actually occurs. Again this objection is quite valid, but again it applies equally to binovular twin pairs with both twins affected, so that the degree of concordance in tumors in binovular twins would be also unduly high. Since the value which is significant

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is the percentage of concordant monozygous twin pairs as compared with the percentage of concordant dizygous twin pairs, these objections have not much weight unless there is a tendency to report twins of the first class more often than of the second.

It is true that monozygous twins occur far more frequently in the series of cases of twins with tumor than they should on the basis of their percentage in all twins. This may be due to several factors. (1) Observers may tend to report such twins more often than the dizygous even when both members of the latter type are affected. Against this may be advanced the fact that the *majority* of medical observers are not sufficiently acquainted with the type of conclusions to be drawn from similarities in monozygous as compared with dizygous twins consciously to select the cases which they think will prove inheritance, and to ignore the others. (2) Since both twins must live long enough to reach the tumor age, a differential death rate operating upon the two types of twins in favor of preserving both monovular twins to the same age while permitting one of the binovular pair to succumb much earlier than the other would account for a higher number of the former living to tumor age. (3) Finally there may be some underlying constitutional anomaly which expresses itself in the splitting of the ovum to form twins in early life, and cancer growth in later life. Whether any or all of these factors operate to produce an undue number of identical twins with tumors is not known at present.

Because (1) twins themselves are not particularly common and (2) the death rate among twins before and immediately after birth considerably exceeds the death rate found in single births; and (3) the chances of *both* twins living to the age at which they could develop the tumor are not nearly so great as the chances of either one of them alone living to tumor age; it is natural that tumors in twins are not met with sufficiently frequently in the practice of any one person to warrant reasonably reliable conclusions being drawn from them. Therefore, it becomes necessary to rely on observations of others in addition to one's own. I collected from the literature 53 pairs of monozygous twins and 35 pairs of dizygous twins in whom tumors had occurred in either one or both twins. The result of this series will be given shortly. In addition, I made an appeal in the *British Medical Journal*, the *Lancet*, the *Journal of the American Medical Association* and the *Canadian Medical Association Journal* for information about tumors in twins. I stressed the point that twin pairs in which only one twin was affected were just as important and as necessary for a correct conclusion as were twin pairs in which both twins were affected. I also asked for a statement as to whether or not the twins were sufficiently alike as to be indistinguishable to their friends. I received 20 case reports from this appeal, 17 of them of cases *not* before included in the literature. Three other case reports were excluded because they dealt with conditions that are not true neoplasias, leaving 14 cases. To these I had three personal cases to add; two of which have just been published, thus making 17 new cases in all. A brief resumé of these new cases will now be given.

Monozygous Twins. Both Affected

Case 1. R. W., male, aged 14 in 1934. Medulloblastoma of the cerebellum removed. Histological diagnosis. Radiation also given, and the patient is still alive in April, 1939.

—— W., male twin to R. W., and so like him that they could not be distinguished, developed a sarcoma of the left scapula at the age of 14 in 1934. The diagnosis was confirmed by a biopsy specimen. The tumor was treated by radiation, and he recovered and is still alive in April, 1939.

Case 2. R. L., aged 7 months. The mother brought this infant girl to the clinic because of poor vision and abnormal eyes. On examination, both eyes were seen to be buphthalmic. Vitreous hemorrhages were present in both eyes so that the retinae could be seen only indistinctly, but it was thought that almost complete retinal detachment existed in both eyes. The right eye was trephined. The mother stated that the infant had a twin sister and she was asked to bring the twin to the clinic which she did the next week.

E. L., twin sister to R. L., aged 7 months. She showed extensive bilateral retinal detachment with occasional minute hemorrhages on

the detached retina. The eyes which were normal in size at the time of the first visit in September, 1937, soon became buphthalmic. In July, 1938, the right eye of R. L. developed a swelling at the site of the trephining, and the eye was enucleated. The pathologist's report was retinoblastoma. R. L. died in September 1938, one year after she was first seen. E. L. developed retinoblastoma also and died several months after her twin sister. They were identical twins.

Case 3. Mrs. F. McC. B., aged 48. She developed a benign tumor of the right breast and later of the left. The nodule was present in the right breast from the age of 40 to 46, when it was removed. The whole left breast was amputated at the age of 44 for benign tumors. She is alive at the age of 48 in 1939, but with pulmonary tuberculosis, and a fibroid uterus.

Twin to Mrs. F. McC. B. has developed benign tumors of both breasts, which have been removed. She is now alive and well. The twins resembled each other so much that their friends were unable to tell them apart.

Case 4. Mrs. H. J. W., developed carcinoma of the stomach at the age of 47. The diagnosis was verified by operation and histological examination. She died at the age of 49.

Mrs. R. T. M., identical twin to Mrs. H. J. W. This fact was established not only through their extreme resemblance, but also by means of the statement of the physician who attended their mother at the birth of twins as to the condition of the membranes. She developed carcinoma of the stomach one month after her twin's first symptom, and died six weeks after the twin. In both, the initial symptoms were those of pernicious anemia. In both, the carcinoma was removed at operation, but both died of pulmonary metastases.

Case 5. Mrs. H., aged 51. She first noted lumps in the *left* breast in December, 1936. She was first seen by her surgeon in March, 1938, at which time a radical mastectomy was performed. The tumor which was diagnosed histologically as a spheroidal cell carcinoma of the breast was situated in the upper and outer quadrant of the left breast. There were no axillary metastases.

Mrs. C., aged 51. She noticed lumps in her breast in 1933, and was treated by a quack cancer doctor. When her sister was operated upon, she came to the same surgeon, and radical mastectomy of the *right* breast was performed in May, 1938. The pathological diagnosis of the tumor was spheroidal cell carcinoma of the breast. Small axillary glands were infiltrated with the carcinoma. The twins could not be distinguished from each other, except with difficulty.

Case 6. Mrs. C. B., aged 59. She had had slight bleeding for six months before she was examined at which time a cervical polyp was removed. She had had a prolapsed uterus for 13 years.

Mrs. L. S., twin sister to Mrs. C. B., was first seen at 64, with a history of irregular bleeding for four years, so that her symptoms began one year after those of her twin. At 68, another cervical polyp was removed, on which there was a histological diagnosis. She had just developed lumps in her breast, which were being irradiated. The first twin had as yet no tumor of the breast. They were diagnosed as identical twins, because of the marked similarity in coloring, features, double arcus senilis in both, identical taste reactions. (This case is included here, since it is a personal case, and had not been published at the time the analysis of published cases was prepared.) Gowing and Macklin.

Monozygous Twins. One Affected

Case 7. T. S., female, aged 8 months. A swelling of the jaw was first noted in December, 1936. At the age of 15 months a pathological diagnosis of fibrosarcoma of the left jaw was made. She was treated and is now alive and well at the age of three years.

L. S., identical twin sister of T. S., has had no evidence of a tumor and is now alive and well at three years.

Case 8. Mrs. A. K., aged 59. She has five children, alive and well. She suffers from essential hypertension, blood pressure being 230/130. At 51, she had hemorrhoids and an anal fissure for which she was treated. At 57, she complained of a recurrence of this condition, and on examination was found to have carcinoma of the rectum. All operative treatment was refused, and she is now in the terminal stages of the disease. A consulting surgeon confirmed the diagnosis of rectal cancer.

Mrs. M. S., aged 59. She has one child. She also suffers from essential hypertension, and her blood pressure varies from 220/120 to 240/130. She has persistent bradycardia with a pulse rate of 45 to 50, while that of her twin is 60 to 70. She too had hemorrhoids which were surgically treated, and at the present has no sign of malignant disease.

Case 9. D. K., aged 28 years, noticed a "pimple" on his arm in 1936. He picked at this, and a lump appeared. This was removed twice, and then lumps began to develop on various parts of his body. The pathologic diagnosis was nonpigmented melanotic sarcoma of which he died in 1937.

The identical twin brother is alive and well in 1939, with no evidence of any similar lesion.

Case 10. H. L., aged 10 in September, 1937, when the cervical lymph nodes began to enlarge. A diagnosis of Hodgkin's disease was made. He was treated by a radiologist in 1939, and is alive and well.

H. L., an identical twin to H. L., also has enlarged cervical glands, but a biopsy specimen revealed no evidence of disease. (This case is not counted in the series of new cases, because there is not a unanimous opinion among pathologists that Hodgkin's disease belongs to the true neoplasias.)

The cases kindly sent in by Dr. Kaplan and Dr. Benedict and Dr. Phillips and Dr. Manjos'

case which Dr. Aikman referred me to, had already been published and hence were included in the cases listed in Table I.

Dizygous Twins. Both Affected

Case 11. M. H., female, born in 1894. She complained of menorrhagia in 1929. Masses attached to the uterus were palpated, and at operation in which a supravaginal hysterectomy was performed, multiple fibroids of the uterus were found, two of them measuring 6 by 5 cm. The pathological diagnosis was "leiomyomata of the uterus and benign hyperplasia of the endometrium."

L. H., female, born in 1894. In March, 1933, she complained of menorrhagia existing during the last five months. A large impacted mass was felt in the cul de sac of Douglas. At operation, when a supravaginal hysterectomy was done, the mass together with the fundus of the uterus was found to measure 9×8×4 cms. The pathological diagnosis was "leiomyomata of the uterus, benign hyperplasia of the endometrium, fibrosis uteri." These women were quite distinct in type, and easily told apart.

Case 12. P. F., male, aged 66. He had a history in February 1938 of sore on the lower lip for six months and swelling of the lower left jaw for two months. The two lesions were quite distinct and separate. The pathological diagnosis was carcinoma of the lip, and carcinoma of the lower jaw. He died in November, 1938.

T. F., male, aged 66. The patient came in in September, 1937, with a history of pain in the ear for two months. He died in January, 1939, with carcinoma of the tongue, fauces, soft palate, and posterior end of the alveolus of the left jaw. The diagnosis was confirmed by histological examination. The patients were similar in appearance, but it was easy to distinguish between the two.

Case 13. A. M., male, aged 58½. He had a history of three months' duration of constipation and loss of appetite. He had hypertension for the last two years. A diagnosis of rectal carcinoma was made, and the patient was operated upon but died seven weeks later. The diagnosis was confirmed histologically.

F. M., male, aged 60, consulted a surgeon, at which time he was discovered to have an operable carcinoma of the rectum. He delayed operation for several months, and died following the operation. The diagnosis was confirmed by histological examination. (This is a personal case, which has been published since the analysis of published cases was made and hence is included here. Macklin and Macklin.)

Dizygous Twins. One Affected

Case 14. W. G., male. He began to have cerebral symptoms in 1932, and in 1937 was operated upon, at which time a tumor the size of an orange was removed from the brain. He is alive and well. The pathological diagnosis was oligodendroglioma.

M. G., female, twin to W. G., has had no symptoms of tumor, up to the present, 1939.

Case 15. B. F., male, aged 11 months. He had cerebral symptoms, which were due to a medulloblastoma. The child was not operated upon, but was given a series of X-ray treatments, which gave great relief. The child is alive at the age of six years, and well.

J. F., male, twin to B. F., alive and well at six years with no cerebral symptoms. The twins are very unlike in their physical appearance, and in their dispositions.

Case 16. J. B. G., male, aged 63 at the time of his death from carcinoma of the stomach, which diagnosis was confirmed at autopsy.

L. F. G., male, twin to J. B. G., died at the age of 60 of what was thought to be chronic nephritis. There was no autopsy, but no symptoms of stomach cancer. The twins did not resemble each other to any extent.

Case 17. A. A. G., son of J. B. G., who was Case 16. He developed carcinoma of the stomach at the age of 46, and was operated upon at the age of 47. A subtotal gastrectomy was performed, and the patient is alive and in good health at the age of 52, 1939. The diagnosis was confirmed histologically.

E. G., male, twin to A. A. G., now aged 52, is alive and in good health, with no signs of gastric cancer. He is quite unlike his twin brother both physically and mentally.

Case 18. Mrs. S. Died at the age of 44 of carcinoma of the middle ear, which was proved by histological examination of the tumor after death.

Her twin sister, quite dissimilar in appearance to Mrs. S., is still alive at the age of 59 and with no evidence of any tumor.

The new cases and the published cases will be analyzed separately, to assess the similarities and differences of the two series. The shorter series of new cases will be considered first. There were nine identical twin pairs and eight fraternal twin pairs, a slight predominance of identical twins. In all of these the diagnosis was confirmed histologically, except in one instance in the fraternal twins. In the identical twin series both twins were affected in six cases or 66.6 per cent; in the fraternal twins, both were affected in three cases or 37.5 per cent. In the identical twin series, both were affected with the same type of tumor in the same site in five cases or 55.5 per cent; while in the fraternal twin series both were affected with the same type of tumor in the same site in three cases or 37.5 per cent (Table II). Thus if one member of an identical twin pair is affected, the second twin is more liable to be affected with the same tumor in the

same situation than if the twins belong to a fraternal twin pair.

The *age of onset* is much more nearly similar in identical twin pairs than in fraternal. Thus in the six identical twin pairs in which both twins were affected, the average difference in age of onset of the tumors was only 0.6 years; whereas in the fraternal twin pairs in which both twins were affected, the average time elapsing between ages of onset was 2.9 years or five times as long.

If the average time is taken during which the second twin has lived without the tumor developing, this value in the monozygous twin series is 2.2 years, while in the dizygous twin series the value is 10.7 years or nearly five times as long. Therefore, identical twins resemble each other much more closely with respect to time of onset of the tumors than do fraternal twins.

Cases from the Literature

From a list of twins, both mono- and di-zygous collected from the literature (Table I) we obtain the following information. There were 53 pairs of monozygous twins. In 32 pairs or 60.4 per cent both showed tumors; and in 31 pairs the tumors were the same, while in one pair the tumors were different. This gives a concordance in type of tumor in 97.0 per cent of the cases where both were affected, and a discordance in 3.0 per cent. But as there were 22 cases dissimilarly affected in the whole group, namely 21 pairs with only one twin affected and one pair in whom the type of tumor was different, there was a concordance of 58.5 per cent in the entire series, and a discordance of 41.5 per cent. (Table II).

In the 35 pairs of dizygous twins, there were 12 pairs or 34.3 per cent with both affected, and 23 pairs or 65.7 per cent with but one affected. One pair of twins with both affected had the second twin showing a tumor dissimilar to that which appeared in the first twin and three years later, a tumor similar to that of the first twin. This pair has therefore been counted twice in Table II and in the discussion which follows, so that the percentages total to more than 100 per cent. In the 12 pairs with both affected, five pairs or 41.7 per cent showed similar tumors or concordance, and eight pairs or 66.6 per cent showed dissimilar tumors or discordance. If all 35 pairs are considered, we have concordance in 14.3 per cent, and discordance in 88.5 per cent. These figures, when compared with the corresponding values for the monozygous twins (Table II), show that concordance is much more frequent in monozygous twins than in dizygous, with respect not only to both twins being affected but also with respect to the same type of tumor occurring in both members of a pair.

It might well be objected here that one reason for the high degree of concordance in monozygous twins in the type of tumor displayed, as compared with dizygous twins, is that half of the dizygous twin pairs are likely to be of different sex. If the female twin develops breast or uterine cancer, the male does not show the same type of tumor. He may have inherited the same genetic factors, but cannot exhibit the outward expression of that inheritance. To overcome that objection, pairs of dizygous twins where both were of the same sex and both affected were selected, and compared with the one-egg twins where both were affected. These were analyzed separately for the published and the new cases, but the total only will be discussed here (Table II). There were 27 pairs of like-sexed dizygous twins, of whom 12 or 44.4 per cent had both affected. Seven of these pairs had similar and seven had dissimilar tumors. This was because two pairs of twins had both similarly affected, and one twin with a second dissimilar tumor. Thus in twins of the same sex but from different eggs there was concordance in the twin pairs in which both twins were affected in 58.3 per cent as compared with 94.7 per cent in the twins from the same egg, with both affected. If concordance be determined for the whole series, the twins agreed in being alike in only 26 per cent in the like-sexed dizygous twins, and in 58 per cent in the monozygous twins.

The difference in age of onset was 9.5 years for the like-sexed dizygous twins, and 1.2 years for the monozygous twins. Therefore, the fact that they were of the same sex and so would have shared a more closely similar environment than would twins of unlike sex was not enough to overcome the possible dissimilarity in hereditary makeup, and these like-sexed dizygous twins resembled each other in both being affected, with the same type of tumor, and at the same age much less closely than did monozygous twins.

In 11 pairs of twins whose condition of being mono- or dizygous is unknown, 10 pairs showed both twins affected with the same type of tumor and with both members of the same sex. In one pair, the sex of the twins was not given, and only one twin was affected. Some of these unknown pairs probably belong to the monozygous group, since there are at least 10 pairs in the series of 11 where the sex was the same.

Similarity in Age of Onset

We have seen that the series of monozygous twins in the unpublished cases agree as to age of onset much more closely than do dizygous twins indicating that this is also partially, at least, determined by hereditary factors. Does the same hold true for the published cases of monozygous twins? In the 32 pairs of monozygous twins, with both twins affected, there was an average difference in age of onset of 1.5 years. The corresponding value for the dizygous twins was 7.8 years or more than five times as great.

Of the 23 cases in which but one monozygous twin was affected, there was an average of 8.5 years between the time of the onset of tumor in one twin and the time of the report at which time the second twin was free. In four cases the report was made as soon as the first twin developed the tumor so that the second twin had not had time to show whether it too would have the tumor. Although it is quite probable that the tumor will develop in some of these instances in the second twin, it is also quite evident that a sufficiently great time has elapsed in some of the cases, to make it improbable that the second twin will develop the tumor. We are then faced with the necessity of explaining the origin of these tumors in one of a pair of monozygous twins, if heredity is the major factor in their causation. First, however, we will take up the age of onset in dizygous twins.

In the 12 pairs of dizygous twins with both affected there was an average difference of 7.8 years between the appearance of the tumors in the twins. In the 23 instances in which but one of the dizygous twins was affected; an average time of 12.6 years has elapsed during which the second twin had remained free from the tumor.

Just as the type of tumor was similar in a far higher percentage of monozygous than of dizygous twins, so the age of onset was much more nearly simultaneous in the one-egg than in the two-egg twins. Thus it would appear that hereditary factors play a strong role in determining not only the presence of the tumor and its type, but also the time of its appearance.

As will be seen from Table II, the same relations hold when the 17 new cases are added to the 88 cases published in the literature.

Sex Ratios in One-Egg and Two-Egg Twins

So far, the twins known to be monozygous have shown definitely greater similarity between members of a pair than have the dizygous twins. One might query at this point as to the value of this group of twins in determining the hereditary nature of tumors. If they had been selected even unconsciously, because similar twins showed similar tumors, they would not be representative of the class of twins in general, and any conclusions based upon them might be false. Have we any way of testing the fact that these twins are random samples of the twin population?

Twins in general occur in the ratio of one pair of male twins to one pair of female twins to one pair of twins of

mixed sex (Arey). Twins which are from two eggs will naturally occur in the ratio of two pairs of like-sexed twins (one pair male and one pair female) to two pairs of unlike-sexed twins. If any condition is dependent wholly or largely upon factors which are resident in the environment, one should find in a series of twins collected because only one twin is affected as well as in a series collected because both twins are affected that the ratio of male twins to female twins to mixed-sex twins should agree with the 1:1:1 ratio found in twins in general. If on the other hand, genetic factors are responsible for the appearance of the trait in question the ratios found in a series in which one twin only was affected would differ widely from the ratios found in a series in which both twins were affected. This would be due to the fact that if the trait were conditioned entirely by inheritance, monozygous twins would both be affected if one were, while in dizygous twins one or both might be affected. Thus in the series in which both were affected there would be a concentration of twins of the monozygous type and hence of like-sexed twins, while in the second group most of the dizygous twins would occur, and hence the unlike-sexed twins would appear as frequently as the like-sexed twins. Conversely, if in a series of twins in which both are affected, there is a predominance of like-sexed twins; and in a series with but one affected, there are as many unlike-sexed pairs as there are like-sexed pairs, then we may conclude that the trait which the twins were exhibiting in common through the series was one dependent largely if not entirely upon hereditary factors.

Perfect agreement with the expected mathematical ratios depends upon several factors. (1) The trait must depend entirely upon genetic factors; partial dependence upon extraneous factors will alter the ratios. (2) The defect must be one which is present at birth, such as a congenital malformation inasmuch as the late appearance of the trait as is the case with tumors, will permit one twin to die before developing the trait or will permit one twin to develop it several years before the second twin shows the same condition. In the latter case, the report might be published before the second twin showed the defect. In this instance, it would be listed as an instance of twins with only one affected. When a large series of twins is collected in which a hereditary defect noticeable at birth affects one or both twins, it will be found that when both twins are affected, there will be a predominance of like sexed twins; and that when one is affected, there will be an approximately equal number of like-sexed and unlike-sexed twins. (Macklin.)

Ratio of Like-Sexed and Unlike-Sexed Twins with One or Both Affected

How do the twins listed in Table I and in the case reports support or deny the role of heredity in tumor production? In the group in which both twins were affected, there were 60 pairs in which the sex was the same, and 3 pairs in which the sex was different. In the 52 pairs in which one twin only was affected, there were 39 pairs of like-sexed and 13 pairs of unlike-sexed twins. Although the first finding is in agreement with the expectation that tumors are dependent upon hereditary factors, the latter finding is not the ratio which we would expect. But of the 39 pairs of like-sexed twins with but one affected, 24 were definitely stated to be monozygous, thus leaving among the known dizygous twins 15 pairs of the same sex. This, with the 13 pairs in which the sex was different in the twins, gives very close approximation to the 1:1 ratio which is expected if heredity were the important factor in producing tumors.

But there remain the 24 pairs of twins, who were stated to be in the monozygous class, but in which one twin only developed the tumor. If tumors are dependent upon hereditary factors, how can we explain this large group of dissimilarly affected one-egg twins? There are several explanations which we will consider briefly.

(1) The tumors in these twins may

have had a predominantly extraneous cause. Such an explanation is quite in accord with what we know of tumor production. One twin working in a dye factory, the other not; one twin working in an X-ray laboratory, the other twin not, would easily give bladder tumors in the first instance, or skin tumors in the second in one twin, while the other remained free.

(2) The second twin may develop the tumor later, and so bring the pair into line with the group in which both members of a monozygous twin pair develop the same tumor. Against this explanation being valid *in all* of these instances, is the much greater average time, namely, 7.5 years which has elapsed without the second twin showing the tumor, than was found between the ages of onset in the cases where both twins had already developed tumors.

But in this series of twins, we found that in one case, as long as eight years elapsed between the times of onset of identical tumors in identical twins. In the 24 cases in which one twin only was affected, there were four cases in which the report was made immediately upon the appearance of the tumor in one twin, no time having elapsed in which the second twin might conceivably develop the tumor. In 12 other cases, the time interval elapsing between the onset of the tumor in one twin, and the time of the report was less than eight years. In four instances only two years had elapsed; in one, 2½ years; in one, 3 years; in one, four years; in two, five years; in three, seven years. It may well be that in some of these, the second twin will develop the tumor later, and thus reduce the number of discordances.

(3) The twins may not really have been monozygous. In families in which the stock is rather homozygous, it is possible that marked resemblances might occur between fraternal twins, although there probably is not much error here.

(4) Some of the twins in this group may belong to that third intermediate group of twins which are supposed to arise from one egg but two sperms. This third type of twin has been recognized by most workers in the field. It arises from the possibility that an egg into which the sperm has entered throws off with the nuclear material of the second polar body a sufficiently large amount of cytoplasm to make a second mature oöcyte. This oöcyte fertilized by a second sperm develops into a new individual. Since the reduction division occurs with the extrusion of the first polar body, this would make the two embryos resulting from this process *monozygous* as far as the *maternal* inheritance is concerned but *dizygous* as far as the *paternal* inheritance is concerned. If the traits determined by the maternal chromosomes were obvious physical resemblances, such twins might resemble one another sufficiently to be regarded as monozygous, but would not be completely so. Their tumor inheritance, for example, might differ, while their physical resemblance was very similar. If some of these twins belonged to this intermediate category, the difference in tumor development might still rest in their case upon a genetic basis, since the line of tumor inheritance might have come through the male.

Twins in this intermediate class could be of opposite sex of course, since that factor is determined by the sperm. No matter how similar such unlike-sexed twins might be as far as physical resemblance was concerned, their difference in sex would at once place them in the dizygous twin class. If they were not particularly alike despite their identical maternal inheritance, they would naturally fall into the dizygous twin class. Hence only those whose physical resemblance depending upon an identical maternal inheritance and who were of the same sex might by mistake be placed in the monozygous twin group. If they differed in their tumor inheritance, or in any other trait supposed to be due to heredity, they would be cited as an argument against the hereditary nature of the trait. As a matter of fact, they would come under the dizygous twin class as far as that trait was concerned. Some or all of these explanations may be involved in the 24 cases of discordance in assumedly monozygous twins.

TABLE II Percentages of Concordance and Discordance in Monozygous and Dizygous Twins.

	1	2	3	4	5	6	7	8	9	10	11	12	12
	No. of pairs	No. of pairs both affected	% of pairs both affected	No. of pairs one affected	% of pairs one affected	No. of pairs both affected similar tumors	No. of pairs both affected dissimilar tumors	% of concordance in twins with both affected	% of discordance in twins with both affected	% of concordance in total series	% of discordance in total series	Average difference in age of onset both affected	Average time in which twin remained free if one was affected
UNPUBLISHED CASES													
Monozygous	9	6	66.6	3	33.3	5	1	83.3	16.7	50.5	44.5	0.6 years	2.2 years
Dizygous	8	3	37.5	5	62.5	3#	1#	100.0#	33.3#	37.5#	75.0#	2.9 "	10.7 "
Like-sexed Dizygous	7	3	43	4	57	3	1	100.0#	33.3#	43	57	2.9 "	8.6 "
PUBLISHED CASES													
Monozygous	53	32	60.4	21	39.6	31	1	97.0	3.0	98.5	41.5	1.5 Years	8.5 years
Dizygous	25	12	34.2	23	65.7	5#	8#	41.7#	66.6#	14.3#	88.5#	7.9 years	12.6 years
Like-sexed Dizygous	20	9	45.0	11	55.0	4#	6#	44.4#	66.6#	20.0#	85.0#	11.7 "	11.8 "
TOTAL													
Monozygous	62	38	61.3	24	38.7	36	2	94.7	5.3	98.0	42.0	1.5 years	7.5 years
Dizygous	43	15	35.0	28	65.0	8#	9#	53.5#	60.0#	13.6#	86.0#	6.3 "	11.7 "
Like-sexed Dizygous	27	12	44.4	15	55.6	7#	7#	58.3#	58.3#	26.0#	81.5#	9.5 "	10.8 "

Summary

1. The cases of tumors in twins reported in the literature, to which 15 new caes hitherto unpublished have been added, are analyzed and the monozygous twins are contrasted with the dizygous twins. It was found that tumors affect both members of a monozygous twin pair far more frequently than they do both members of a dizygous twin pair.

2. The tumor is of the same type and occurs in the same organ in both twins far more frequently in the monozygous than in the dizygous twin group.

3. The age of onset of the tumors is much more nearly identical in the monozygous than in the dizygous twins. These three findings emphasize the fact that heredity plays an important role in tumor production and age of onset.

4. The instances in which monozygous twins show a dissimilar tumor history do not destroy the above conclusion, but suggest that several other explanations may be involved, namely:

a. Extraneous factors may sometimes play a predominant role in tumor production, and

b. Apparent discrepancies in the appearance of a supposedly inherited trait in monozygous twins may be due to a faulty diagnosis of their monozygosity. The twins may be really dizygous, but come from a homogeneous stock so that they may appear very similar; or they may be of the intermediate variety derived from one egg but two sperms, so that the maternal inheritance is identical, while the paternal differs. Such a group might easily be confused with monozygous twins when the twins are of the same sex, if mere physical resemblance be relied upon to diagnose monozygosity.

c. In the cases in which one twin only is affected, it may well be that the second twin will develop the tumor at a subsequent date, thus increasing the percentage of concordance between the twins. This may also hold in the case of some of the dizygous twins, but the much greater lapse of time in the case of dizygous twins during which the second twin has remained free of tumor, makes this less likely.

TABLE I.—Tumors in Monozygous Twins

Author	Sex	Affected	Type of Tumor	Age of Onset	Remarks
Abelin[1]	Female	Both	Congenital cystic kidneys	Present at birth	Double monster with mirror imaging of defect
Allen	Female	Both	Popliteal cysts	9, 9	
Bardach[1]	?	Both	Naevus		
Becker[2]	Female	Both	Myoma of uterus		
Benedict	Female	Both	Retinoblastoma	4, 4½	
Birkenfeld[2]	Male	Both	Cartilaginous exostoses		One on tibia One on femur
Burkard[1]	Female	Both	Adenofibroma of breast	21 in both	
"	Female	Both	Carcinoma of breast	Simultaneously	
Champlin	Male	Both	Sarcoma of right testis	24 and 31	One had history of injury, other none
Croom	Female	Both	Myoma and adenocarcinoma of uterus in both	54 and 54	
Hutchinson	Female	Both	Naevus of lips	Simultaneously	
Joughlin	Female	Both	Glioma of brain	32, 36	
Kaplan	Female	One	Carcinoma of breast		Second twin had been free for 2 years
Kimbrough[1]	Female	Both	Carcinoma of ovary		
Kranz[3]	Female	Both	Fibroma of breast	46 and 49	One twin had myoma of broad ligament also.
"	Male	Both	Cancer of lip	38 and 41	Recurrences at 47 and 48 respectively
"	Male	Both	Cancer of pylorus Cancer of rectum	58 60	Difference in site
"	Male	Both	Cancer of bile duct, duodenum and pancreas	64, 64	Died within 2 days of each other.
"	Male	One	Cancer of rectum	69	Second twin well at 74.
"	Male	One	Cancer of stomach	51	Second twin well at 67
"	Male	One	Tumor of brain		
"	Male	One	Lymphosarcoma of mediastinum	39	Second twin well at 44
"	Male	One	Cancer of stomach	31	Second twin well at 38
"	Female	One	Cancer of bladder	73	Second twin well at 80
"	Female	One	Osteosarcoma of leg	19	Second twin well at 69
Leavitt	Male	Both	Cerebeller glioma	6½, 8½	
Leers	?	Both	Neurofibroma		
Lucksch	?	Both	Teratoma of frontonasal region	Present at birth	
MacFarlan	Female	Both	Facial naevus	Not given	
Millitzer	Male	Both	Cancer of stomach	70, 70	Symptoms began within month of each other. Both had bilateral inguinal hernia
Munford	Female	Both	Carcinoma of upper left quadrant of left breast in both	89, 89	Both seen at 91

TABLE I.—Tumors in Monozygous Twins (Continued)

Author	Sex	Affected	Type of Tumor	Age of Onset	Remarks
Phillips	Female	Both	Primary tumors in right and left breasts in each twin. Ovarian in one twin		Difference of onset 3 years
Rosanoff	Female	Both	Cancer of right side of face		Died within a few months of each other
Twinem	Female	Both	Ovarian cyst	16, 17	
Spannocki[3]	Female	Both	Myoma of uterus		
Verluys	Female	Both	Both had uterine myoma and neurofibromatosis	59, 59	
"	Female	Both	Myoma of uterus		
"	Female	One	Congenital sarcoma of tibia		Second twin well at 3 yrs.
"	Female	One	Neurofibromata	36	Second twin normal at 56
"	Female	One	Cancer of ovary		Other twin normal
"	Male	One	Cerebellar glioma	31	Other twin normal at 35
"	Female	One	Myoma of uterus	43	Other twin well at 50
"	Male	One	Lymphadenoma of orbit	9	Other twin well at 11. Both were idiots
"	Male	One	Branchiogenic cancer	70	Other twin well at 70
"	Female	One	Hemangioma	5 mos.	Other twin without tumor at 5 mos. Both had umbilical hernia
"	Male	One	Hemangioma	3 yrs.	Other twin without tumor at 3 yrs.
"	Female	One	Hemangioma	16 mos.	Other twin without tumor at 16 mos.
Waaler	Male	Both	Cancer of stomach	53, 54	
"	Female	One	Cancer of breast	59	Other twin well at 71
"	Female	One	Cancer of breast	?	
"	Female	One	Cancer of breast	?	
Weitz	Female	Both	Myoma of uterus		
"	Female	Both	Cancer of uterus	39	Died at 39 of cancer
			Myoma of uterus	44	Died at 47 of Cancer of liver, supposed to be due to metastases from uterine cancer
			TUMORS IN DIZYGOUS TWINS		
Kranz[8]	Male Female	Both	Cancer of larynx Cancer of breast	?	Both died on same day
"	Female	Both	Cancer of rectum Cancer of breast	67 68	
"	Female	Both	Cancer of stomach Cancer of uterus	70 77	
"	Female	Both	Multiple tumors Cancer of ovary	2½ 28	
"	Female	Both	Cancer of ovary Cancer of uterus	24 47	
"	Female	Both	Myoma of uterus Cancer of breast	37 37	Also myoma of uterus at 40

TABLE I.—Tumors in Dizygous Twins (Continued)

Author	Sex	Affected	Type of Tumor	Age of Onset	Remarks
"	Female	One	Cancer of rectum	50	
	Male				Well at 70
"	Female	One	Tumor of hypochondrium	57	
	Male				Well at 69
"	Male	One	Cancer of parotid	41	Other twin well at 42
"	Female	One	Cancer of uterus	34	Other twin well age not given
Siegel	Female	Both	Lymphatic leukemia	2½, 2½	
	Male				
Verluys	Male	Both	Cancer of stomach		
"	Female	Both	Cancer of breast		
"	Female	Both	Hemangioma		
"	Male	One	Cancer of stomach		
"	Male	One	Hemangioma		
"	Male	One	Sarcoma of kidney		
"	Female	One	Cancer of breast		
	Male				Unaffected
"	Female				Unaffected
	Male	One	Melanoma		
"	Female	One	Fibromyoma of uterus		
	Male				Unaffected
Waaler	Female	Both	Cancer of stomach	?	Died in 1914
			Cancer of vulva	?	Died in 1928
"	Female	Both	Fibrosarcoma of ovary	?	Operated on in 1910, living in 1930
	Male		Cancer of stomach	?	Died in 1910
"	Male	One	Cancer of stomach	61	Other twin well at 89
"	Male	One	Cancer of stomach	47	Other twin died of renal condition at 55.
"	Male	One	Cancer of liver	76	Other twin died of unknown cause at 88
"	Female				Well at 54
	Male	One	Cancer of stomach	50	
"	Male	One	Cancer of skin	50	
	Female				Well at 70
"	Female	One	Cancer of breast	54	
	Male				Well at 70
"	Male	One	Cancer of stomach	60	
	Female				Well at 61
"	Male	One	Sarcoma of vertebra	60	
	Female				Well at 75
"	Male	One	Cancer of stomach	61	
	Female				Well at 76
"	Female	One	Cancer of breast	40	Other twin well at 47
"	Female	One	Cancer of breast	71	Other twin well at 77
"	Female	One	Cancer of breast	50	Other twin well at 70
"	Female	One	Cancer of stomach	47	
"	Male				Well at 63
	TUMORS IN TWINS NOT STATED TO BE MONOZYGOUS OR DIZYGOUS				
Gesell[3]	?	Both	Skin tumors		Question as to their monozygosity but of same sex

TABLE I.—Tumors in Twins (Continued)

Author	Sex	Affected	Type of Tumor	Age of Onset	Remarks
Gould[3]	?	Both	Chondrous exostoses		Question as to their monozygosity, but of same sex
Kranz[3]	Male	Both	Cancer of stomach		
Lucksch	?	One	Teratoma of sacrum		
Peacock	Male	Both	Hodgkins disease		
Silcock	Female	Both	Sarcoma of eye	38	Other twin had had eye removed for some reason other than injury
Stocks-Barrington[3]	Male	Both	Exostoses		
Szontagh[1]	?	Both	Papilloma of larynx	?	Of same sex, but some question as to their being monozygous
Touraine	Male	Both	Angiomatosis of brain	10	Both had internal strabismus and epilepsy
Wells	Male	Both	Seminoma		Personal communication to Twinem
Wilder[3]	Female	Both	Cancer of breast		Were two of triplets; other triplet, female, without cancer

[1]Indicates references cited by McFarland and Meade.
[2]Indicates references cited by Verluys.
[3]Indicates references cited by Peyron and Kobozieff.

AUTHOR'S NOTE: I wish to express to the following physicians my warmest appreciation for their response to my appeal for data on tumors in twins, and for giving me permission to include these cases in this report: Dr. L. M. Davidoff of the Jewish Hospital, Brooklyn for Case 1; Dr. Laurence Duncan of Melbourne, Australia, for Case 2; Dr. F. B. Robinson of Oxford, Pennsylvania, for Case 3; Dr. Geo. L. Rohdenburg of New York, for Case 4; Mr. A. Kirk Wilson, of the Royal Liverpool United Hospital, Liverpool, England, for Case 5; Dr. H. W. Jacox, of the Western Pennsylvania Hospital, Pittsburgh, Pa., for Cases 7 and 10; Dr. L. Inglis Johnston of Carlisle, England, for Case 8; Dr. Wm. Steinberg of Philadelphia, Pa., for Case 9; Dr. M. H. V. Cameron of Toronto, Canada, for Case 11; Dr. A. A. D. La Touche, of the Radium Department of the General Infirmary at Leeds, England, for Case 12; Mr. W. G., who is the patient in Case 14, and Dr. K. G. McKenzie of Toronto who sent me the pathological report on the tumor; Dr. Geo. I. Dawson of Napa, Cal., for Case 15; Dr. P. L. Gray of S. Brooksville, Maine, for Cases 16 and 17; Dr. R. H. J. Swan, of London, England, for Case 18; I also wish to thank Dr. I. I. Kaplan of New York and Dr. W. L. Benedict of Rochester, Minn., for sending me the report of their cases of tumor in twins which I have included in the published cases; Dr. J. Aikman of Rochester, N. Y., for calling my attention to the report of Drs. Allen and Manjos, which I had not seen, and which I included in the published cases. Finally, I wish to thank Dr. Glenn Bell of San Francisco, Ca., for his report on pyloric stenosis in twins, and Dr. J. B. Jobin, of the Hotel-Dieu, Quebec for his report on toxic goitre in twins. I did not include either of these or the case 10 sent in by Dr. Jacox, as they did not fall into the category of true neoplasia, although each one was in the true sense of the word a tumor or swelling. To all of these I express my gratitude for sending me the information on their cases.

After the above paper had been sent in for publication, three other cases of tumors in twins were sent in. They are not included in the totals or the percentages in the above analyses, but their inclusion would in no wise alter the conclusions reached on the other cases. A fourth case came to my attention, and I wish to thank Mr. H. A. for permission to include the data on Mrs.

E. C. A., and also to thank Dr. Chas. Gray of Tampa, Fla., for the radiological notes on this patient. The cases are as follows, and I thank the physicians who sent them in. (1) Dizygous female twins, one of which was operated upon for hypernephroma May, 1939. The other twin was alive and well April, 1940. Reported by Dr. Jane Armstrong of Northampton, Mass. (2) Dizygous female twins, one of whom died at 3½ years of a reticulo-cell tumor of the left maxilla. The other twin is alive and well at 4½ years. Reported by Dr. L. Judelsohn of Buffalo, N. Y. (3) Monozygous twins, females. Both had cervical polyps at 40 and 41. Reported by Dr. E. W. Nicklas of Washington, D. C. (4) Dizygous twins, females. One twin developed rectal carcinoma, and was operated upon, but died of pulmonary metastases 5 years later. The other twin is alive and well 5½ years after her twin's first symptoms, and without any signs of rectal cancer. Radiological notes forwarded by Dr. C. M. Gray of Tampa, Fla.

2. I wish to express to Prof. Charles C. Macklin my gratitude for his unfailing interest in my work, and for the opportunities which he gives me to continue my genetic studies in his laboratory.

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INHERITANCE OF PINKEYE IN THE FOWL

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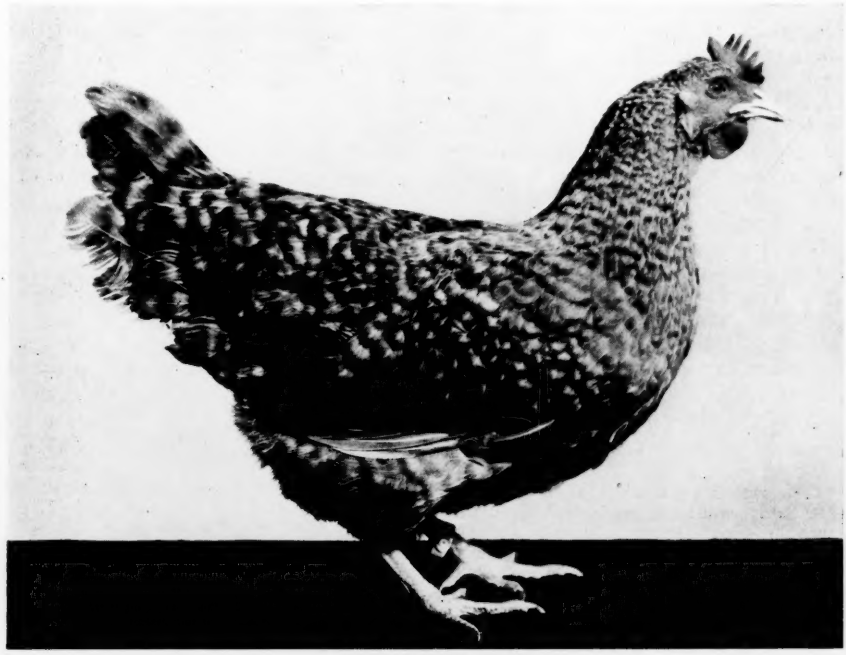

A "PINKEYED" HEN

Figure 7

The pinkeye condition is due to a single recessive gene. The pinkeye gene affects eye pigmentation in the same way that the albino gene does, but the pigmentation of the feathers is not so greatly affected, though it is definitely reduced.

THE original pink-eyed birds in this study were a trio of White Plymouth Rocks sent to Kansas State College by a private breeder.† They appeared to be similar to the albino White Wyandottes earlier reported by the writer‡ and it was first thought that the two cases were due to the same factor. Chicks hatched from the original three birds showed the same transparent pink eye exhibited by the albino chicks.

No homozygous albino birds were available at the time that the pinkeyes were received but the pinkeye male was mated to three normal-eyed heterozygous albinos. In a total of 46 offspring, no pinkeyed chicks appeared, indicating that these were independent factors, since had they not have been, half the chicks should have been pink-eyed.

In Table I are given the results of additional matings of the pink-eyed stock.

*Contribution No. 127, Department of Poultry Husbandry.

†These birds were received from Mr. Carl W. Beck, Sterling, Colo.

‡D. C. Warren. Inheritance of Albinism in the domestic fowl. *Journal of Heredity* 24: 379-383. 1933.

The results clearly demonstrate that pinkeye is a simple recessive character. There was considerable shortage of pinkeyes in the F_2 generation but the expected equality of pinkeyes and normals was realized in the backcross generation.

Additional proof that factors pinkeye and albino were two distinct factors was found in the F_2 generation where pinkeyes appeared with colored plumage. A large number of albino birds have been reared in the past few years and in no case did any of them possess plumage color other than white indicating that they were probably true albinos.

A number of the pinkeye segregates showed various color patterns. In all cases, however, the black in the pattern was diluted, giving blue. Similar relationships already have been observed among mammals. Thus the factor responsible for pinkeye, when homozygous, affects the plumage color. The colored pinkeyes when mature seemed to have somewhat darker eyes than did those possessing white plumage. Like albino fowls, the pinkeyes have defective sight and are difficult to raise.

It, therefore, appears that pinkeye in the fowl is a simple recessive eye color factor. Its effect on the eye color is apparently identical with that of the albino factor but its influence on plumage color is unlike that of albinism. Pinkeye tends only to dilute the melanic pigment in the plumage pattern exhibited by the bird.

Table I. Inheritance of Pinkeye.

Mating	Daughters		Sons		Total	
	Normal	Pinkeye	Normal	Pinkeye	Normal	Pinkeye
F_1	149		134		283	
F_2	82	13	48	13	130	26
Backcross to pinkeye	93	74	45	62	138	136

"Race Hatreds"—Cultural or Genetic?

The criteria upon which racial differences are based require careful examination. One such criterion, for example, which plays a very considerable part in the popular evaluation of racial differences, is the criterion of physiognomy. This criterion, for its proper evaluation, requires the closest collaboration between the physical and the social anthropologist. Among white skinned peoples the face is perhaps the most important criterion utilized by most individuals in making a judgment of another individual's "race." Such judgments are expressed in the familiar "He looks English," or "Jewish," or "German," and so on. Such judgments are not entirely subjective, for there can be little doubt that the quality of looking English, or Jewish, etc., within certain limits, actually represents something real. This quality requires analysis. It is highly probable that the set of an individual's face, his expression, is as culturally determined as are his habits of manual gesticulation. The habitual expression or set of an individual's face to a large extent represents the expression of the kinds of cultural influences which have been operative upon him, and wihch manifest themselves in the "gesticulations" of his muscles of expression. Clearly one uses one's muscles of expression in the expression of ideas and emotions in a manner which is significantly determined by cultural factors; and it depends to a considerable extent upon the cultural environment in which an individual has been brought up and educated whether he will "look like" a Harvard man or a hobo, an Englishman, a Jew, or a German. * * * *

It has often been said that racial hatred between men will disappear only when all physical racial differences have been obliterated in a common melting pot; but it may also be argued that the most potent source of racial hatreds are not physical differences but cultural ones, and that if this is so one would be justified in arguing that racial enmities will disappear only with the obliteration of all cultural differences between men. But this would constitute as doubtful an argument as the former. The world would be immensely the poorer for the dissolution of all cultural differences between people. There is one belief, however, which is an essential article of faith and practise of many peoples, expressed in the notion: "We are the best, all others are our inferiors; they must be kept in their proper place." I deem it among the most important tasks of the anthropologist to make available the evidence which may eventually prove to all mankind whether any people has, or has ever had, any justification for such a belief other than an hypertrophied sense of its own importance and the wishful mythology they have devised to support them in this belief. Only when the combined labors of physical and social anthropologists have laid bare the true meaning and value of such beliefs will anthropology have justified its claim to be called the science of man. —W. F. Ashley-Montagu, in *Amer. Jour. of Phys. Anthrop.*, March, 1940.

GENETIC STUDIES OF COLEUS

II—Leaf Form and a Bud Mutation

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NOT only are leaf color variations abundant in *Coleus blumei,* but a number of distinct types are also found in regard to leaf form and flower parts. Two of such variations, deep versus shallow leaf lobes, and crinkly versus smooth leaf surface are not uncommon.

A plant characterized by deeply lobed leaves was observed in a commercial greenhouse in New Orleans. In color, the leaves were green, with an albino midrib. The plant was brought to Columbus, and was kept in the greenhouse a year and a half before blooming. The flowers produced no pollen, having abortive stamens, as shown in Figure 9*C'*. This plant was crossed with several plants having shallow lobed leaves. Of the progeny, ten are characterized by deep, and sixteen by shallow lobed leaves. Six of the plants with deep leaves have bloomed, and all are male sterile, like the female parent. Eight of the plants with shallow lobed leaves have bloomed, and all have normal stamens (see Figure 9*A'*), producing fertile pollen.

Another plant having deeply lobed leaves was observed on a porch. The leaves were green, with a brown pattern on the upper epidermis. The plant was purchased and taken to the greenhouse. Upon blooming, it was also found to be male sterile, having abortive stamens. It was crossed with a shallow lobed heterozygous purple plant[2] of genotype *PpIi* (*P*=extension of purple, *pp*=restriction of purple to a pattern: *I*=intense green chlorophyll, *ii* = dilute green chlorophyll). Two progeny were produced, both intense green with a pattern. One had deeply lobed leaves and was male sterile, whereas the other had shallow lobed leaves and was male fertile. The plant with deeply lobed leaves was crossed with a plant of genotype *PpIi,* having shallow lobed leaves. The following progeny were produced:

7 deep lobed leaves, purple
4 deep lobed leaves, intense pattern
1 deep lobed leaf, dilute pattern
2 shallow lobed leaves, purple
1 shallow lobed leaf, intense pattern
1 shallow lobed leaf, dilute pattern

Seven of the plants with deeply lobed leaves have bloomed, and are male sterile; two of the plants with shallow lobed leaves have bloomed and are male fertile. Apparently the variation in leaf lobing is due to a single pair of genes, the gene for deeply lobed leaves being dominant. As such plants are male sterile they must of necessity be heterozygous. When crossed with plants having shallow lobed leaves we should expect equal numbers of plants with deeply lobed and plants with shallow leaves among the progeny. Table I summarizes the data and shows a close conformity to a 1:1 ratio.

Beijer,[1] in studies of *Coleus hybridus,* also found deeply lobed leaves to be due to a dominant gene, but he reported no instances of male sterility.

Crinkly Leaves

A large green plant with crinkly leaves, resembling lettuce, was obtained from a commercial greenhouse. When crossed with a purple, smooth leafed plant a single purple, rough leafed offspring was produced. (See Figure 9*B.*) Upon selfing, an approximate 9:3:3:1 phenotype ratio appeared, as shown in Table II. Thus crinkly leaves appear to be due to a simple dominant gene, not linked with the gene (*P*) for the extension of purple. We shall use *C* to designate the gene for crinkly leaf, and *L* to

*The writer wishes to acknowledge his indebtedness to the Botany Department for greenhouse facilities.

A MUTANT PLANT

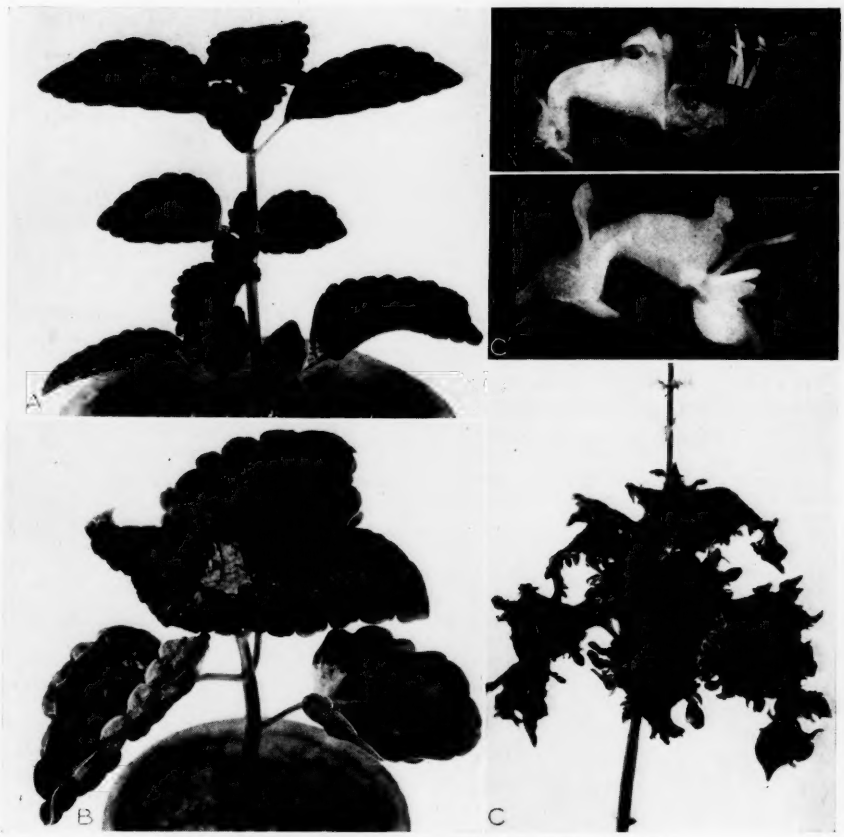
Figure 8

Mutations in leaf color are exceedingly rare in coleus. One branch of this plant has a mutant branch with dark leaves, due to the presence of anthocyanin. The mutant form was lost before it could be propagated, and it never came into flower.

designate the one responsible for deeply lobed leaves.

A Bud Mutation

Stout,[3] in reporting his investigations of Coleus, concludes that variability is as great in lines propagated asexually as in those grown from seed. Yet strains of Coleus under observation here, propagated vegetatively for periods ranging from four to ten years, have never shown a single clear-cut mutation in leaf color. However, a plant showing a striking bud mutation was observed in a commercial greenhouse three years ago. The plant was purchased and placed in our



LEAF FORM AND MALE STERILITY IN COLEUS

Figure 9

A—shows a purple, shallow lobed, smooth leaved plant; and *A'*, its flower. *C* shows a purple, deep lobed, smooth leaved plant. The deep lobed type of plant has male sterile flowers without stamens (*C'*). A purple, shallow lobed, crinkled leaved plant is shown at *B*. This shape of leaf is due to a dominant factor.

Botany greenhouse. The dark branch (see Figure 8) presumably arose from the mutant bud. Originally the mutant branch was about three inches above the surface, the photograph having been taken after the plant was repotted. The darker color of the mutant branch is due to the presence of anthocyanin in both the upper and lower epidermis of the leaves. Leaves over the entire plant have a wide albino spot in the leaf center, and also flecks of anthocyanin extending through the mesophyll tissue.

Cuttings were made from both branches, but due to the comparatively small amount of chlorophyll, great difficulties were encountered in maintaining the lines. The mutant line unfortunately was lost without ever having bloomed. After two years the non-mutant stock produced progeny. All were phenotypically similar to the parent, none like the mutant. It seems likely, then, that the color of the dark branch was due to

a dominant mutation. We have not observed the phenotype of the mutant in any other lines.

Summary

A dominant gene (*L*) is responsible for deep lobes in leaves, the recessive condition resulting in leaves with shallow lobes. All observed plants of genotype *Ll* are male sterile.

Crinkly leaves are caused by a dominant factor (*C*), plants of genotype *cc* having smooth leaves. *P* and *C* appear to segregate independently.

A dominant bud mutation resulted in the presence of anthocyanin in both the upper and lower epidermis of leaves.

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2. Boye, C. L. and D. C. Rife. Genetic Studies of Coleus I. Leaf Color. *Journal of Heredity* 29:55-60. 1938.

3. Stout, A. B. The Establishment of Varieties in Coleus by the Selection of Somatic Variations. Carnegie Institute of Washington Publication No. 218. Washington, D. C. 1915.

Table I. Progeny of deep x shallow leaf lobes

Types	Actual	Expected
Deep	23	22
Shallow	21	22
Total	44	44

Table II. F_2 Progeny of purple smooth x green crinkly

Types	Actual	Expected
Purple Crinkly	90	87.75
Purple Smooth	28	29.25
Green Crinkly	26	29.25
Green Smooth	10	9.75
Total	150	150

✦

AMERICAN MEDICINE IS TOLD ABOUT GENETICS

ONE of the curious facts about the progress of genetics is that relatively very few medical men are interested in the subject. This is odd, because as our knowledge has grown, it has become increasingly evident that in diagnosis, the study of disease, and the prognosis of many medical conditions depends in many instances on the heredity of mutations as one important item. Failure to realize this fact has left medical theory hanging somewhat like a modern "Mohammed's coffin," 'twixt heaven and earth.

A new day seems to be dawning. The pharmaceutical houses and the commercial research laboratories are undertaking to enlighten those reluctant practitioners of medicine who are still in ignorance regarding the wonderland of the gene. First there was a discussion of colchicine in the house organ of the Abbott Laboratory, which did an excellent job — well illustrated — explaining how chromosome division works, what polyploidy is and how it comes about.

Soon after that a much more ambitious venture in a genetic publication was undertaken by the Dial Ciba Corporatiton. This was consummated in a 48-page booklet relating the history of genetics, the basic chromosomal mechanism of Mendelian inheritance and the modern developments in genetic science.

Since genetic research in America today is equal to that done in any other country, the source of the material used in this booklet is worthy of comment. It was written by a presumably *Swiss* woman, Dr. Anne-Marie Du Bois. The original *French* text was translated into *English* and it was illustrated with a number of plates — most of them *American.* A regrettable angle of this exposition is that *no sources whatever are given as to the origin of the illustrations.* Several of them are from the Journal of Heredity. It is highly unfortunate that *there is no bibliography* which would enable the practitioner of medicine to go on after he had come to page 48, if he desired to continue his study of genetics in general and of medical genetics in particular.—R. C.

THE RESPONSIBILITY OF PARENTHOOD*

GEO. W. KOSMAK, M.D.
New York City

THE original purpose of this Association was to convince prospective mothers that prenatal care was essential. This ideal is now generally accepted and it has led to a wider vision with respect to maternity, of its responsibilities to the welfare of the race and the nation and to an acknowledgment of the need of adequate preparation for parenthood, both social and physical.

Parenthood has been looked upon too frequently as the natural, perhaps accidental, accompaniment of marriage. In a sense it is, but have we regarded childbearing in its wider aspects, including its broadly economic, social, and political phases?

We have been made conscious, particularly in recent years, of the great importance of an adequate birth rate to national prosperity and success. Perhaps we should not view this in the light it is viewed abroad. We need not look upon an abundance of babies as possible later additions to the armies of conquerors. Nevertheless, we need in this country a supply of babies to balance the constantly expanding group of older people, for in the last few decades the span of life has been increased by twenty years. Naturally there are difficulties. We cannot broadcast merely the Biblical injunction—to go forth and multiply; much more than a process of multiplication is involved. Now, if I am not careful, I will begin to preach and that is a role for which I am not adapted. I do not feel, moreover, that preaching alone will do much good, perhaps teaching would be more desirable.

We have given a great deal of thought to the products of marriage but have we done enough in the preparation for this important event? I do not mean in a social and material, but rather in a physical, psychologic and eugenic sense. In the troubled state of the world outside of our own borders, the minds of people are agitated by many other questions, nevertheless their writings still disclose a deep and lasting interest in matters of population maintenance and the disturbance of family life by war and its consequences. But we in this country are still separated from foreign strife by two wide oceans and while our economic life undoubtedly is upset, we are more or less free to think and to do as we desire. So we can think of marriage and parenthood both in an abstract as well as in the personal sense. It is unlikely that the dictates of Cupid will fail to continue as the guiding influence in marriage. However, if we are to believe his accepted picture, he is shown usually as a mere happy child. Although he may be an expert with the bow and arrow, has he shown any ability to guide the future of the victims of his archery? After the arrow has reached its mark, much in marriage seems to be left to chance. If we assume that childbearing is the most natural outcome of marriage, have the two contracting parties given this sufficient thought? Are they mentally, physically and otherwise capable and have they taken any steps to find out? The marriage ritual asks the question but is the answer based on actual knowledge of the facts? Some of our States, in response to certain demands, have made a blood test the basis for obtaining a marriage license. Will it be necessary in the future before the marriage vows are legalized, to submit a certificate showing freedom from other transmissible diseases, or from physical or mental defects?

Perhaps we would resent such an inquisition at the present time although the progress of eugenic ideas may bring about a more liberal attitude in the fu-

*Address, in part, presented at the Tenth Annual Mother's Day Meeting of the Maternity Center Association of New York.

ture. In the meanwhile and as a step to the other, can we not agree among ourselves to consider more carefully the implications of marriage as these bear upon hoped for maternity? I feel that the Maternity Center Association can further the movement in this direction by favoring and perhaps initiating some form of marriage consultation service among its projects. This is not an entirely new field, such centers have been instituted elsewhere in a modest way but there is need for establishing techniques and standards and for educating people to resort to such facilities.

All of this may seem a radical departure from accepted forms, it may appear like an interference with our free thought and actions, it may savor of the totalitarian ideologies of certain foreign governments. Perhaps it would be if it were imposed upon us by legal statute. But cannot we anticipate the latter by developing our own social status in these respects without any feeling of compulsion, but rather of sense and reason? In other words, parenthood must be given an additional meaning, its practitioners must be thoroughly informed, their responsibilities towards childbearing must be clearly appreciated. Much has been done to make maternity safer, there need be no fears nor terror.

Homes Are Still Necessary

Preliminary to the bearing of children we need a proper conception of the word home. The great changes in our economic structure, the struggle for existence in a narrowed sphere of activity, the entrance of woman herself into business as distinct from industry, the desire of young people to begin at the top rather than at the bottom or even at the middle, the unsettled condition of our social and political status, have all contributed to the problem of making a home. A proper home is essential to a proper family life, children cannot be brought into a homeless world with advantage or safety. Children not only must be born, they must be properly cared for. A home must serve as the chief agency of defense in the struggle against the many outside influences of the present day which have a tendency to disrupt rather than contribute to home making.

However, I am not a pessimist, I do look hopefully towards a future when most of the difficulties to which I have referred will be ironed out. This country remains the world's greatest experimental laboratory. We have the means and the natural resources and we have the energy to do great things, as must be evident from our past. But we need youth and unless we can supply and maintain an adequate crop of babies and healthy mothers to take care of them, we must submit in time to those nations who will devote themselves to this task. It is a strange thing that in those countries which have labelled themselves as democracies, there should have occurred such notable declines in birth rates as compared with those which we somewhat derisively have regarded as autocratic. Can we not take one small leaf out of their book with some advantage to ourselves? A more practical view towards parenthood and the means to make it successful in this great country of ours, should not prove an unsurmountable task. I look upon the altruistic efforts of voluntary organizations such as the Maternity Center Association as an outstanding means to accomplish this much-to-be-desired end, for I am convinced that more can be accomplished by the molding of public opinion than by the institution of arbitrary measures through other agencies.

I feel that in the course of time parenthood, in a sense, will become a selective process, not that it is to be regulated by contraceptive or birth control measures indulged in as a matter of convenience, but rather by a selection of fit parents to whom childbearing shall be a matter of duty towards their race. This means a willingness to assume parenthood, to ensure that this will be properly conducted and supervised, and that preparations for its successful outcome shall be a matter of mutual acceptance, not an accident, but rather a design.

Education for parenthood has been largely neglected. In attempting to for-

ward it we have often limited it to the teaching of sex and thus have failed to achieve the object which we really have sought. The family life and the home should constitute the basis of a much more satisfactory knowledge of the responsibilities which our youth as individuals must in time assume. We devote much time in our high schools and colleges to provide what is usually a smattering of science, of literature, of the devious terms of the calculus, of the sex-life of bees and butterflies rather than of home-making and possible parenthood. In my personal contacts as a physician I have often been astounded at the lack of knowledge among prospective mothers about babies, and the higher the academic degree, the less a woman seems to know. Of course knowledge comes with experience but the learning of the lesson is often fraught with difficulties and an unfortunate outcome. Perhaps the day will come when our educational authorities may view the need in its proper light and provide less algebra and more instruction in the make-up and functioning of our bodies, so that we may know how we live and why we live. The facts are simple enough but we must acquire them and appreciate them for the sake of ourselves and of the coming generations.

THE SIGNIFICANCE OF THE MODERN PROGRAM OF EUGENICS

EUGENE L. BELISLE
Executive Director Massachusetts Mothers' Health Council

WHEN society does not shape and pursue a well defined program of eugenic development, the diverse influences and pressures affecting human behavior as regards marriage and the creation of children work haphazardly, therefore almost inevitably dysgenically. These same influences and pressures are capable of *reorganization* in such a way as to minimize negative and dysgenic results, and encourage positive and eugenic results.

Such reorganization implies reorientation of minds, indeed, reorientation of souls.

Primary significance of a modern eugenic program is its philosophical influence—its emphasis on the dialectical union of organic-spiritual forces which comprises the entity, individual man, and out of which all technology, culture, economic and social systems are born and continued anew, as determined, at least within an area of choice, by each living and new generation.

This philosophical influence is primary because it says: "In the reconstruction of society, we must begin with man and not with his works. The human problem lies in perfecting the organic-spiritual structure of men, who are the foundation of all other structures in the world society. To old experience, modern science adds immense new knowledge for the achievement of this task."

In a free society, as contrasted with a totalitarian state, the reorientation and reorganization required for eugenic development must be accomplished by permitting individuals their free and voluntary choices in action, beginning with actual conditions rather than awaiting more favorable hypothetical conditions in the future. Fortunately, the most important and most numerous factors affecting eugenic development may be effectively reorganized by educating men, not necessarily the majority, as to what their possible choices are and what the results of these choices will be, starting from present, given situations.

Despite the seeming lack of interest in such education, immense numbers of people actually thirst for it. Most human misery of the most intimate and

(*Continued on page* 303)

THE EFFECT OF CHILDREN ON THE ECONOMIC STATUS OF AMERICAN FAMILIES

FRANK LORIMER*
American University

IN earlier agricultural societies children were a source and symbol of prosperity, "like olive plants round about thy table." The writer's father, a little more than fifty years ago, felt the urge of higher education. His father, being a Godly man, after due consideration granted this son "his time"; no further aid, beyond this contribution, was asked or given. Today, the presence of children automatically lowers the family's plane of living at any given income level; in many cases it also involves a lowering of family income through the wife's withdrawal from the labor market. Severe family limitation has therefore become associated in contemporary society with other means of achieving a higher level of family living.

This article presents an analysis of certain data on the relation of economic status to number of children among native-born, non-relief American families, comprising only husband, wife and varying numbers of children under 16 years. The data are derived from the Federal Study of Consumer Purchases, 1935-36, conducted by the Bureau of Labor Statistics, Bureau of Home Economics and National Resources Committee, in cooperation with the Works Progress Administration and the Central Statistical Board.

Variations in expenditure patterns in relation to number of children are here discussed for native white families sampled in three areas: Chicago, three middle-sized cities in the East North Central area, and certain farming communities in Ohio and Pennsylvania. In order to compare expenditures when income is held constant, proportional expenditures were computed for each family type at each income level and these proportions were applied to "standard populations" — representing the distribution by income class of all families sampled within each area — to give "standardized proportional expenditures" by family types for various categories of expense.†

The most striking change in proportional expenditure, in passing from families without children to families with one, two, and three or four children, appears in the food item. At given income levels the proportions of the total family expenditure allotted to food rise consistently in all areas as the number of children in the family increases. The data for Chicago are shown in Figure 10, with an increase from 31.1% for families of husband and wife only to 38.6% for families with husband, wife and three or four children under age 16. In the farm group, taking money expenditure plus value of foods produced for home consumption in relation to the total value of family living, the proportion rises from 35.8% for families of husband and wife to 42.8% for the families with three or four children. This increased expenditure for food at given income levels must obviously be offset by curtailment of expenses on other items. This curtailment

*Based on studies made by the Seminar on Population, the American University Graduate School. Data from published reprints and special tabulations by Bureau of Labor Statistics and Bureau of Home Economics. The author accepts full responsibility for development of data and interpretation.

†LORIMER, FRANK, and HERBERT ROBACK. Economics of the Family in Relation to Number of Children. *Milbank Memorial Fund Quarterly*, April, 1940, Vol. XVIII, No. 2, pp. 119, 120. The reader is referred to this article for statistical results and further interpretation.

is not consistently evidenced for any particular group of expenditures (except decrease in gifts and taxes which offset about one-third or one-fourth of the increases for food) but appears at various points, notably household operation, furnishings and equipment, and transportation.

Lowered Food Consumption

In spite of the increased expenditure for food the average food consumption level is lowered as number of children increases. It should be noted, however, that "food" includes meals purchased away from home, candy and beverages. Taking customary variations in expenditures for persons at different ages into account, the average expenditure per food cost unit (per meal, per adult equivalent) decreases from $.246 in the families without children to $.144 in the families with three or four children in Chicago. (See Figure 10.) There is a corresponding but somewhat less sharp decrease in the middle cities and in the farm group. Among farm families the value of food consumed (including that grown for home consumption) decreases from $.138 per food cost unit for the families without children to $.092 for families with three or four children. The differences remain large when the estimated value of the service factor in meals outside the home is taken into account. Thus it appears that as the number of children increases families compromise between increased food requirements and other added needs by devoting a larger part of their available resources to food but nevertheless accepting a more restricted diet than they would otherwise be able to afford.

If all family types invested the same proportion of their total income in food it would appear that, in order to maintain the same consumption level, families with three or four children in Chicago would need just about twice the income of childless families, and even farm families of this size would need more than 50% larger incomes than childless families in the same area.

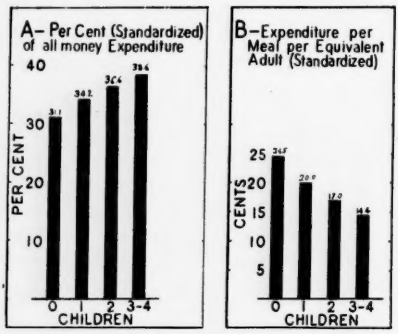
In urban families the proportions spent for clothing are not very different among the various family types considered in this analysis — the added expenditures for children's clothing being offset by reduction in the expenditures of husband and wife for their own clothing. Thus in the Chicago sample, 8.9 per cent of the family income is used for the clothing of husband and wife in families without children, but only 4.9 per cent in the case of families with three or four children.

The increased housing needs of families with children are just about balanced by increased financial strain, especially the necessity for expenditure of larger amounts for food, so that the proportional expenditure for housing, relative to total expediture for all items, remains fairly constant among the various family types. Obviously this means the acceptance of more crowded quarters or movement to less satisfactory locations.

The number of children also affects the relation of total expenditure to income or, conversely, the margin of savings or liability to debt. Urban families without children or with only one child show on the average small but substantial net savings during the period covered by the study (3 per cent to 5 per cent of money income as the standardized mean of proportional net savings). On the other hand, in both urban series, families with two or more children had on the average a very narrow margin between income and expenditure (less than 1% of money income). In such families the deficits of those who were unable to make both ends of the family budget meet just about cancelled the savings of those who kept expenses below current income.

No detailed analysis has been made, in the case of most items, of how the relative expenditures of different family types may vary at different income levels. Such an analysis, however, has been carried out with reference to expenditures for food by Chicago families.*

*Unpublished paper by Richard M. Graham.



CHILDREN AND THE FOOD BUDGET

Figure 10

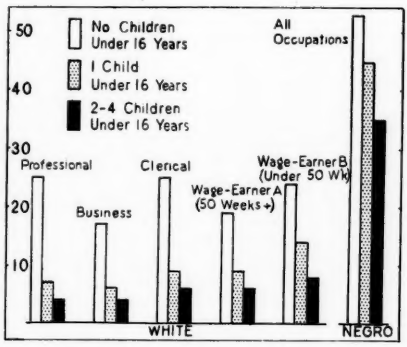
Expenditure for food by families of husband, wife and specified number of children under 16 years. Data from Consumers Purchases Study, Chicago. From: Lorimer, Frank, Ellen Winston, and Louise K. Kiser, *Foundations of American Population Policy,* Harper and Bros., 1940. (Advance release.)

The differences between each pair of family types, both as regards proportional expenditure and expenditure per food cost unit, remain fairly constant within the income range $500 to $5,000 per year. Apparently as income levels rise standards of living in the care of children, at least as regards expenditure for food, also rise at a fairly uniform rate, until we reach income classes which include only a small fraction of the population.

Employed Wives

The presence of children has an equal, perhaps even more striking effect on the gainful occupation of wives outside the home. In this analysis families with two, three or four children are treated as a single family type. The proportion of wives reporting earnings (not including income from boarders or lodgers) are computed separately for three age classes (wives under 30 years, 30-39 years, and 40 years and over) and for five occupational groups of native white families classified according to the occupation of the principal earner. The wage-earner families are divided into two groups, (*A*) those in which the principal earner was employed 50 weeks or more and (*B*) all others. A Negro sample is included but without occupational classification. The whole sample used in this analysis includes all qualified families in the cities surveyed by the Bureau of Labor Statistics, a total of 112,000 families. Variations in the proportions of wives 30-39 years who report earnings, classified by race-nativity, occupation and family type, are shown in Figure 11. It is apparent that there are large differences in the proportions employed between wives in families without children under 16 years of age and mothers with one or more children in the family. The differences in the frequency of wives' employment, within any occupational class, between families with one child and families with two to four children under 16 years are small but significant. There is very little difference in the frequency of wives' employment, when the occupation of the principal earner and family type are held constant, between wives under 30 and wives 30-39 years. On the other hand, the employment drops sharply, except in the case of Negroes, when wives over 40 are compared with wives 30-39 years in families without children under 16 years. It must be borne in mind that in many cases the older wives in this family type may have raised children who are no longer dependents within the family.

These figures show the prevalence of a strong tendency in contemporary society for many women to continue employment after marriage so long as there are no children in the family, but to drop employment and thus sacrifice this factor in maintaining the economic status of the family when they become mothers. Under strong economic pressure the need for continuing wife's employment is undoubtedly a major motive leading to the postponement of initial pregnancy and perhaps not infrequently to abortion. This delay obviously reduces the childbearing span and may appreciably increase the proportion of women who remain permanently childless. The extent to which delay in initial pregnancy or resort to abortion may increase sterility is a subject on which there is at present



EARNINGS OF WIVES

Figure 11

Percent of native-born urban wives, aged 30-39 years, reporting earnings by race, occupation of principal earner and family type. Source: same as Figure 10.

little conclusive evidence. This is urgently needed for further clarification.

Among responsible parents, able to control the processes of reproduction, childbearing is a high privilege and responsibility. It is properly motivated primarily by personal considerations and affection, sometimes with an added sense of religious or social responsibility. Any attempt to force or bribe people to have children would be either ineffective or dysgenic in its effects. It is nevertheless essential in the interests of sound population policy and eugenics that aid and support to the family be established so that parents need not, as at present, jeopardize the standards of living which they set for themselves and their children if they wish to add new lives to the family.*

There are two possible approaches toward equalization of levels of living among families with none or few children and large families. One approach is that of the family allowances, proportional to number of children, without regard to income level. The other approach is that developed in the Swedish population program, a system of special service to families with children and subsidies in kind. The latter approach makes possible more direct contribution to child welfare and tends to provide more proportionate aid at all moderate income levels (whereas the former affords greater proportional aid at the lower income levels), but this system is dependent on the development of large scale, intelligently directed public enterprise and services. The problem of reconciling childbearing functions with occupational interests and earnings of wives is even more difficult but no less important. Perhaps progress can be made in this direction through a greater development of opportunities for part-time employment, adequate provision for maternity leaves, with pay, nursery school provisions and other aids. Possibly, if sustained economic recovery can be achieved, young wives may feel less urgent need for full time, profitable employment, and a larger proportion may wish to place primary emphasis on children in the family. These considerations open up large new fields of population policy in which there is need for further research, experimentation and enlightened public interest.

*For full treatment of this subject see the following books now in process of publication by Harper and Bros.: Osborn, Frederick, *Preface to Eugenics;* Lorimer, Frank, Ellen Winston and Louise K. Kiser, *Foundation of American Population Policy.*

✦

The Modern Program of Eugenics

(*Concluded from page* 299)

painful character—psychical maladjustments, inharmonious marital and family relationships, sexual nostalgias, numerous organic ills, marginal vitality in all aspects of living—flows directly from the social lag, the lack of personal and social orientation. But people are becoming increasingly conscious of their thirst, partly because, like Tantalus, they begin to see the rich fruits and sparkling

elixir of life just out of ordinary reach.

Such initial eugenic proposals as sex education for youth, preparation for marriage, marriage adjustment counsel, are cases in point. Their importance is greater than first appears. Their effects are doubly therapeutic: they tend to heal basic personal maladjustments that may cause dysgenic results; they also establish influences toward selection of better marriage partners, the gradual control of venereal diseases, the more intelligent planning and spacing of children, and other results tending to strengthen the organic endowment of offspring as well as the immediate family environment of the new generations.

Unspectacular though these developments in eugenic education may be, they represent a gain of solid ground which is more significant than, let us say, achievement in sterilization of hereditary defectives. For the recreation of individuals and families is a positive gain in the creative vitality of society. It is an unfolding of undeveloped human potentialities.

And it is this vitality alone which will give meaning and strength to human endeavors in other realms—the reconstruction of our cities, the improvement of general economic well being, the integration of social relations.

We need only observe the magnificent housing projects of Europe, many of them now ruined in civil or international war, to realize how central to the solution of all problems must be a program of eugenic education, if new economic, social and political constructions are not to prove to be an empty shell.

Number of Two-Child Families Increasing

An analysis of the national birth rate for the years 1920 to 1937 was presented at the June Meeting of the Population Association of America by Dr. A. J. Lotka, Metropolitan Life Insurance Company statistician. This analysis showed that since 1933, the year of the lowest numerical point in the birth rate, there has been a sharp increase in the number of first births to a point exceeding that of any other year since 1925. This evidence of a greater number of family groups is accompanied by a definite increase since 1933 in the number of second children born. A halt in the decline of third births is indicated; and for the last two years included in Dr. Lotka's study, the number of third children born has remained about stationary. The increase in the first two orders of birth contributed to a definite rise in the birth rate for the total population between 1933 and 1938.

P. K. Whelpton, of the Scripps Foundation for Population Research, read a paper on "Future Trends and Differentials in Prolificacy Distribution," which dealt with studies of the 1920 birth registration area for the years 1919 to 1921 and 1929 to 1931.

In his summary Professor Whelpton stated that the practice of contraception is frequently cited as the chief reason for the declining birth rate. All couples do not wish to prevent births entirely, he pointed out, "many merely wish to lengthen the period between marriage and the first birth, and to control the number of subsequent children and their spacing."

"A severe depression may result in postponement of marriage, and of first births by couples already married, on a sufficiently large scale to raise significantly the proportion of childless women in a prolificacy table for a base weighted with such depression years," Professor Whelpton said. "The opposite situation may arise during an ensuing period of prosperity, when the postponed first births occur in addition to those normally expected."—*Information Service*—Birth Control Federation.

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20-75. Extra Fingers—Recessive Polydactylism.
20-74. Chart Showing Inheritance of Polydactylism.
20-352. A Folding Man — Hereditary Absence of Clavicles.
22-104. Crooked Arms and Legs—Hereditary Bone Fragility.
23-307. Five Generations of Glaucoma.
23-425. Inherited Doublejointedness in the Thumb.
24-269. Chart Illustrating Sex-Determination.
22-47. Palm Prints of Fraternal Twins.
22-40. Corresponding Fingerprints of 3 Pairs of Identical Twins.
23-369. Identical Twins—Separated 13 Years.
23-2. Identical Twins Reared Apart—Mary and Mabel when Grown.
23-5. Identical Twins Reared Apart—Mary and Mabel as Babies.
23-6. Identical Twins (Mary and Mabel) at Two Years—Reared Apart.
23-8. Palm Prints of an Identical Twin (Mary).
23-9. Palm Prints of an Identical Twin (Mabel)
24-443. Mirror-Imaging in Eye Dominance in Identical Twins.
24-444. Table Classifying Handedness in Twins.
25-140. Table of Physical Characteristics of Identical Twins Reared Apart.
25-141. Identical Twins (Harold and Holden) Reared Apart.
25-293. Retina of Normal and Albino Eyes.
25-332. Abnormal Fingers and Toes — Syndactyly, Camptodactyly, and "Hammer Toe".
25-333. Short Finger (Brachymetapody) due to short metacarpal bone.
25-391. Hereditary Absence of Incisors.
25-446. Inherited Chest Depression—Koilosternia.
25-483. Sex-Linked Toothlessness.
25-338. Fifty-fifty Segregation for Woolly Hair Mutation.
26-143. Extra Toes—Inherited Polydactylism.
10-389. Twin Acrobats—Identical Twins.
10-409. Typical Identical Twins.
20-50. Identical Twins Reared Apart.
20-52. Palm Patterns of Identical Twins.
20-155. City Boy and Country Boy—Identical Twins Reared Apart.
20-160. Enlarged Prints of Left Ring Finger of Identical Twins (C & O).
22-202. The Hilton Siamese Twins.

ANIMAL

16-154. Pelvic Bones of Normal and Rumpless Fowls.
16-352. Leg Bones of "Reptilian Fowl" and of Normal.
19-413. A Fertile Mare Mule.
19-414. "Old Beck" (Mule) and Her Horse-Like Colt.
19-415. Mule-Like Colt of Fertile Mare Mule.
20-112. Recessive Leg Feathering in Fowl.
20-113. "Heel Tufts" in Fowl.
21-323. Heterogeneous Chromosome Structure — Irregular Cell Division in Drosophila Melanogaster.
22-213. Conjoined Twins in Cattle.
22-296. Backcrosses between Hybrid (Zebu × Yak) Female × Zebu Male.
22-298. Zebu Bull Used in Yak Crosses.
22-299. Yak Cow Used in Zebu Crosses.
22-305. A Zebu-Yak Hybrid—4 Years Old.
22-305. Zebu-Yak Hybrid Cow—4 Years Old.
23-449. A "Flightless" Rooster.
24-183. Parallel Color Mutations (Himalayan and Siamese) in Rabbit and Cat.
25-208. Five Generations of Inbreeding—Shorthorn Cattle (chart).
25-266. X-Ray Photos of Normal and "Flightless" Feathers in Fowl.
25-435. Production of Agglutinins in Rabbits (chart).
25-432. Agglutination of Incompatible Bloods.
25-464. "Portraits" of Salivary Gland Chromosome.
25-466. Giant Chromosomes Compared with "Normal".
25-469. Synapsis of Normal and Inverted Chromosomes.
25-470. Synapsis of Deleted and Normal Chromosomes.
25-472. Cyto-Genetic Maps of Salivary Chromosomes. (Insert.)
26-62. Salivary Chromosomes and Gonial Chromosomes Compared.
26-62. Reference Map of the Salivary Chromosomes of Drosophila. (Insert.)
26-182. Structure of Salivary Gland Chromosome.
26-301. Effects of Sex Hormone Injection on Feather Growth.

PLANT

15-204. Capsules of Globe Daturas.
17-372. Awn of Manchuria (Rough) Barley.
17-373. Awn of Lion (Smooth) Barley.
18-128. Adaptive Cupped Tendrils of a Plant.
18-129. Adaptive "Vacuum Cup" Toes of a Toad.
18-382. Leaves of Live Oak × Post Oak Hybrids.
18-383. Leaves of Overcup × Live Oak Hybrids.
19-419. "Fatback" and "Razorback" Honey Locust Pods.
20-24. Haploid and Dwarf Diploid Tomato Plants.
20-25. Haploid and Normal Tomato Plants.
20-26. Haploid and Diploid Tomatoes.
20-206. Flax Hybrid Produced by Artificial Nutrition.
22-368, 370. Plant Patents No. 2 and No. 3.
24-131. Radium-Induced "Ridged" Tomato.
24-133. Chimeral Radium-Treated Tomato Leaves.
24-134. Radium-Induced Virescent-White Tomato Seedlings.
24-368. Gene Mutation Chimera in Delphinium.
26-137. Ears of Maize, Relatives, and Hybrids.
25-448. Fasciated Sugar Beet.

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